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Science Policy Study—Hearings Volume 18
NATIONAL RESEARCH FUNDING LEVELS

HEARINGS

BEFORE THE

TASK FORCE ON SCIENCE POLICY

OF THE

COMMITTEE ON
SCIENCE AND TECHNOLOGY
HOUSE OF REPRESENTATIVES

NINETY-NINTH CONGRESS

SECOND SESSION

APRIL 15, 16, 1986

[No. 113]

Printed for the use of the
Committee on Science and Technology

ORIGINAL

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CONTENTS

WITNESSES

April 15, 1986:

Page

- Dr. Harvey Brooks, Benjamin Peirce Professor of Technology and Public Policy, John F. Kennedy School of Government, Harvard University, Cambridge, MA 1
  - Prepared statement 7
  - Gerald M. Edelman, "Profits and Prophecy: A Partial View of American Science Near the Millennium," prepared for a conference at the John F. Kennedy School of Government, Harvard University, Cambridge, MA 20
  - Harvey Brooks, "Comments on Gerald Edelman's Paper," March 12, 1986 56
  - "Britian's wasted research," *The Economist*, January 17, 1981, p. 83 65
  - Discussion 66
- Dr. Martin Neil Baily, Senior Fellow, The Brookings Institution, Washington, DC 70
  - Prepared statement 81
  - Martin Neil Baily and Alok K. Chakrabarti, "Innovation and U.S. Competitiveness," *The Brookings Review*, Fall 1985, pp. 14-21 98
  - Discussion 106
- Dr. John W. Kendrick, professor of economics, The George Washington University, Washington, DC 108
  - Prepared statement 112
  - Discussion 121

April 16, 1986:

- Dr. Nicholas Rescher, university professor of philosophy and director of the Center for Philosophy of Science, University of Pittsburgh, Pittsburgh, PA 125
  - Prepared statement 131
  - Discussion 141
- Dr. Eugene Garfield, president, Institute for Scientific Information, Philadelphia, PA 145
  - Prepared statement 149
  - Discussion 158
- Dr. Christopher T. Hill, Senior Specialist in Science and Technology Policy, Congressional Research Service, Library of Congress, Washington, DC; accompanied by Joan D. Winston, Senior Research Assistant, Office of Senior Specialists, Congressional Research Service, Library of Congress, Washington, DC 162
  - Prepared statement 175
  - Discussion 196

Appendix: Additional Materials for the Record:

- Nestor E. Terleckyj, "Economic Effects of Government R&D Spending in the United States," Executive Summary of a Final Report to the National Science Foundation on the Project PRA 81-18098, NPA Discussion Paper, August 31, 1984, National Planning Association, Washington, DC 201
- Nestor E. Terleckyj, "Economic Effects of Government R&D Spending in the United States," Final Report to the National Science Foundation on the Project PRA 81-18098, NPA Discussion Paper, August 31, 1984, National Planning Association, Washington, DC 217
- 1983 SCI/SSCI C5 map 345
- 1984 SCI/SSCI C5 map 346

Page

Appendix—Continued
- Historiograph tracing research leading to the monoclonal antibody technique ... 357
- Eugene Garfield, "Third World Research: Mapping Science in the Third World," *Science and Public Policy*, June 1983, pp. 112-127 ... 358
- Nobelists in the 300 most-cited authors, 1961-76 ... 374
- Nobel Prizewinners on 1,000 most-cited authors list ... 375
- Eugene Garfield, "Citation Classics—Four Years of the Human Side of Science," reprinted from *Current Contents*, No. 22, June 1, 1981, pp. 5-16 ... 376
- Eugene Garfield, "The 1,000 Contemporary Scientists Most-Cited 1965-1978. Part I. The Basic List and Introduction," reprinted from *Current Contents*, No. 41, October 12, 1981, pp. 5-14 ... 388
- Eugene Garfield, "The 1,000 Most-Cited Contemporary Authors. Part 2A. Details on Authors in the Physical and Chemical Sciences and Some Comments about Nobels and Academy Memberships," reprinted from *Current Contents*, No. 9, March 1, 1982, pp. 5-13 ... 398
- Eugene Garfield, "The 1,000 Most-Cited Contemporary Authors. Part Details on Authors in Biochemistry, Biophysics, Cell Biology, Enzymology, Genetics, Molecular Biology, and Plant Sciences," reprinted from *Current Contents*, No. 21, May 24, 1982, pp. 5-13 ... 407
- Eugene Garfield, "The 1,000 Most-Cited Contemporary Authors. Part 2C. Details on Authors in Hematology, Histology, Immunology, Microbiology, Physiology, and Virology," reprinted from *Current Contents*, No. 22, May 31, 1982, pp. 5-13 ... 416
- Eugene Garfield, "The 1,000 Most-Cited Contemporary Authors. Part 2D. Details on Authors in Cardiology, Endocrinology, Gastroenterology, Nephrology, Neurobiology, Neurology, Neuropharmacology, Nuclear Medicine, Oncology, Pathology, Pharmacology, Psychiatry, and Surgery," reprinted from *Current Contents*, No. 24, June 14, 1982, pp. 5-13 ... 425
- Eugene Garfield, "The 1,000 Most-Cited Contemporary Scientists. Part 3. Details on Their Current Institutional Affiliations," reprinted from *Current Contents*, No. 27, July 5, 1982, pp. 5-20 ... 434
- Eugene Garfield, "The 1984 Nobel Prize in Medicine Is Awarded to Niels K. Jerne, César Milstein, and Georges J.F. Kohler for Their Contributions to Immunology, reprinted from *Current Contents*, No. 45, November 11, 1985, pp. 3-18 ... 450

NATIONAL RESEARCH FUNDING LEVELS

TUESDAY, APRIL 15, 1986

House of Representatives,
Committee on Science and Technology,
Task Force on Science Policy,
Washington, DC.

The task force met, pursuant to call, at 10:25 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the task force) presiding.

Mr. Mineta. I call the Science Policy Task Force to order.

This morning the Science Policy Task Force begins 2 days of hearings on "National Research Funding Levels." During these hearings we will review an important aspect of our national science policy—at what level the Federal Government should provide funds for science and scientific research. We will explore what criteria should be used to determine that level and whether or not an optimum level can be determined for support of science.

During the last 40 years, Federal support of science has grown more or less without interruptions, with an annual budget increase of about 10 percent. We now should review this policy critically to determine whether or not it still is appropriate.

Today, we as a nation are facing a period of severe budget restraints. In this situation it is extremely important that we determine national science goals to the best of our ability and allocate funds to meet those goals.

This morning we welcome and are pleased to have Dr. Harvey Brooks, professor of technology and public policy at Harvard University; Dr. Martin Baily, a senior fellow at Brookings Institution; and Dr. John W. Kendrick, professor of economics at George Washington University.

Dr. Brooks, we have your statement and it will be made a part of the record; and if you would go ahead and proceed in your own fashion, sir.

STATEMENT OF DR. HARVEY BROOKS, BENJAMIN PEIRCE, PROFESSOR OF TECHNOLOGY AND PUBLIC POLICY, JOHN F. KENNEDY SCHOOL OF GOVERNMENT, HARVARD UNIVERSITY, CAMBRIDGE, MA

Mr. Brooks. Thank you, Mr. Chairman.

With your permission I will not try to read from my statement but will, in fact, approach the subject in a somewhat different way, referring to parts of my statement when I get to that point. But what I would like to do is to take the agenda that your committee has set out on pages 41 to 45 of the task force agenda and address certain of the questions in parts B and D of that section.

The first question: Should all good scientists be supported? The answer to that is probably not. It has never been the case. In any case, the use of the word "good" in the question, it seems to me, begs the question. What I would argue is that it is more important that the best scientists be adequately supported than that all good scientists be supported.

Most important scientific advances are produced by a small minority of the active research scientists as evidenced by such quantitative indices as citations, numbers of papers published, and so on, and recognition by peers; and, I think, the priority in science policy should be given to identifying and supporting the best scientists.

The difficulty with this is, of course, that the definition of the best is not easy, and one reason we support a rather large number of scientists is there is a lot of mobility within the population of scientists, whenever we have to support a large enough number so we can keep identifying and reidentifying the best.

The education and investment in scientists is certainly not wasted. In fact, in almost every other field, such as the arts and music and so on, we train a great many more people than ever earn their living in those fields. Well-trained scientists are better at doing many things besides science than less well-trained people. I believe that science is a good general education for many other kinds of work in our society, so I don't think the mere fact that some scientists, well trained-scientists, are not engaged in research, or engaged in science, necessarily means that the resources spent on their training have been wasted. What fraction of scientists should be supported, of course, is another question that I will touch on a little bit more later.

The second question is: To what extent should expected social benefits of advances in new technologies and new pharmaceuticals determine overall funding levels for science? Clearly the answer is yes to some extent, in principle. The difficulty, of course, with this answer is that it's difficult to apply in practice because expected benefits are the most difficult thing to predict and often take a long time in appearing. Just to mention one example, witness the payoff now just beginning from the enormous Federal investment in molecular biology and biochemistry in the 1960's and 1970's. This payoff was perhaps predicted in general, but it was certainly never envisioned in its specifics when the investment was made.

Predictions of expected payoff are, I believe, of dubious validity as a basis for allocating investments in science. They certainly should be considered among other factors, but they should not be given more weight in the science budget than what I would call scientific opportunity; that is to say, the prospect of the conceptually significant advances in knowledge independent of potential applications.

I think one can make a pretty good generalization that good science eventually gets applied. However, there is another important point, I think, that ought to be mentioned in connection with this question, because in a certain sense there may be an implication behind the question that the benefits for investment in science are automatic. I think it is important to note that the realization of the benefits from our scientific investments depend heavily on complementary investments in capital, in marketing, in all kinds of other activities. A colleague of mine just before I came down called my attention to an article published in *The Economist*, January 17, 1981, about Sir Alan Cottrell. It's an interview with Sir Alan Cottrell which is headed "Britain's wasted research," and the point of the article is that—it refers entirely to industrial research—the point of the article is that only about a sixth of the industrial R&D in Britain, he estimates, is really getting utilized because, in fact, the capital investments and the other complementary investments by industry in Britain are simply not large enough to embody and take advantage of the other five-sixths of the R&D.

I think there is considerable evidence that that is also a problem in the United States, that specifically the utilization of technology is much less effective for a whole variety of reasons in the United States than it is in some of our strongest competitors, particularly Japan. In fact, at one point in my written statement I point out the fact that, when that is the case, there is a danger that our investment in basic research, or in research more generally, will benefit our competitors more than ourselves, and, I think, this is something that we have to worry about.

Next there are really three questions, all of which are related to the question of trying to establish some base level of support for science. Now, a prime difficulty with this, of course, is what the committee means by science and scientific. In fiscal 1986, the Federal R&D budget was $60.7 billion. I do not believe one could consider that all science. In fact, a very large part of that is defense development or DT&E, development, test, and evaluation as it is called, which uses scientific resources, but really is not science.

I presume what the committee means by the support of science is the support of research for the purposes primarily of advancement of knowledge for a number of purposes rather than knowledge for highly specific purposes. I think the support of the generation of knowledge for highly specific purposes and particularly development has to be considered in the light of a particular end use objective which is being supported; and, in particular, I would say that major development projects, particularly in defense but also projects such as the space station, should not compete within the science budget or be charged against science.

On the other hand, other large projects such as, for example, the superconducting supercollider, or very large radioastronomy array, or even an applied satellite are legitimately chargeable to science.

In my written statement I try to make an estimate of what the science budget is, and I think what we are talking about is something no less than $8 billion a year, and probably no more than $12 billion a year—somewhere in that range—and so, I think, these questions could really only be meaningful if what we were talking about is that $8 to $12 billion.

It is my belief that a base level of support for science—and, of course, it is very difficult to define because if you redefine just a little bit of development as science, you can change the amount of money you are spending for science by 50 percent—I point out if only 10 percent of the Government's development money really could be considered as science, that is 50 percent of this $8 billion figure that I mentioned. But my fear is that any attempt to establish a base level of support is likely to be quickly converted from a floor into a ceiling. I think a base level would discourage scientific entrepreneurship, the kind of scientific entrepreneurship which has always been one of the strengths of U.S. science.

In a certain sense I agree with the premise of the committee that some kind of a base is needed, but I think it would be a mistake to recognize it explicitly as a matter of policy, and I think pretty much the same thing applies to any sort of figure such as percentage of gross national product or annual growth. The danger is these things become frozen in concrete and that floors be converted into ceilings.

On the sixth question, that of a stable number of grants, I believe this may be somewhat more worthwhile considering as a policy because largely for the reason that it tends to decrease, reduce the large amount of decoupling which exists in our present system between logistic support for science and personnel commitments for science in the universities. However, the downside of this policy is that it runs the risk of going counter to my advocacy of concentrating resources on the most productive scientists in times of stringency. If you decide on a given number of grants, the tendency, the strong temptation is if resources are very constrained is to keep—sort of equalize between the grants, and I think this would be counterproductive from the standpoint of the productivity of the investment in science.

On the question of curtailment of funding, the seventh question, that is a very complicated issue which I discuss at considerable length in my written statement. I think the answer to the question as it is stated is very difficult to predict. It depends on how the cut is managed, and there are some fundamental issues such as whether the cutting is done on the basis of allocations to individuals or institutions, or on the basis of allocations to fields. If the management of the cuts were turned over to the leaders of the scientific community, I think the outcome would be the least damage to science, but probably the most political unhappiness across the whole polity, including a large part of the scientific community. I discuss that in considerably more detail in my written statement.

Let me turn to section D. I will skip over the section of the financial health of the universities.

And the first question in that is—should the United States—should the Federal science funding include the aim of keeping the United States first in every field of science? I think again the answer is probably not. Different fields of science do have different importances, but I think the question of—there are sort of two levels of support that one can think of for each field of science. One is the level of support necessary to keep the United States at the forefront, and the other is the level of support necessary for the U.S. scientific community to be able to monitor the ongoing progress in science in the world. I would say in each field of science we need to invest at least enough for that monitoring function, but I do not think necessarily for keeping us in the forefront. I guess, again, I would tend to answer that question more in terms of people in institutions than in terms of fields of science. If there are people in institutions that are at the forefront of world science in whatever field, I think we should try to nurture those institutions and keep that lead.

There is a second question: What can be done to eliminate wasteful duplication? I guess I tend to deny the premise of that question, that there is a great deal of wasteful duplication. The one point I would make in this regard, which I think is important, is that the U.S. system of peer review, one of its functions, one of its indirect benefits—and it also has serious costs as I'll mention in a moment—but one of its very great indirect benefits is the elimination of wasteful duplication because it does have the effect of exposing many active scientists to research plans in a field of science, and I do think it does tend to, therefore, reduce duplication.

The final question is: Has limited funding prevented the support of innovative research proposals? I believe this is a problem, and it is a problem with the peer review system as it's presently implemented, combined with constrained resources, and it's a problem that is likely to get worse as resources become more constrained. In this connection I would refer you to my written testimony, and particularly the supplement that I am distributing with it, which is a slightly edited version of some comments I made on a speech by Professor Edelman of Rockefeller University which he recently gave at Harvard. The point of this commentary is that I think that in some areas—and I would mention specifically condensed matter physics which is, of course, my own field and one that is very important for our industrial competitiveness—the scientific productivity of people in a few of the best industrial laboratories is greater and especially with respect to more innovative, the most innovative, long-term projects in those laboratories—and I singled out Bell Laboratories and the IBM corporate laboratories, but I think there are other examples—than the corresponding research that is going on in universities.

And one of the reasons for that is that what we have gradually developed in the United States and one of the down sides of the peer review system is we have developed what I call the "hunting license system" of doing research. A university appoints a young man or a senior man to a professorship or to a junior faculty position and offers him essentially stimulating colleagues and good students, but beyond that he is offered only a hunting license to try to get support for his work; and I think gradually over the years that has become a very, very wasteful process, in part because the time, the lead time from the time you make your research plans to the time you get funding for them is getting longer and longer. Now I think it averages something like 18 months. I don't mean 18 months between a proposal and funding, but 18 months between the time you have the idea and the time you actually can begin spending resources on it, the difficulty in changing directions in the light of new discoveries and so on.

So I think we need—what I argued for really in the statement is that we need to think not about pluralism in sources of support, which I think we now have, but pluralism in mechanics of support. I think we have become too exclusively dependent on the project grant mechanism of support; and if you go back to the 1960's, you will see we had much more diverse mechanisms in terms of support for individuals, support for institutions and so on. So I think this is something that needs reexamination.

Mr. Chairman, that's been a very brief runover of the questions. I refer you to my written testimony for further details on some of these points, but I think I will conclude here.

[The prepared statement of Dr. Brooks follows:]

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April 13, 1986

FEDERAL SUPPORT OF SCIENCE AND SCIENTIFIC RESEARCH

Testimony of
Harvey Brooks
Benjamin Peirce Professor of
Technology and Public Policy
John F. Kennedy School of Government
Harvard University*

Definitions

Total federal support for R&D, including facilities, exceeded $60 B. in the FY1986 budget, of which 72.3% was for defense and 27.7% for non-defense R&D.[1] However, there is a question of how much of this should be included under the rubric of "science and scientific research" which this committee is studying. If one splits up both defense and non-defense R&D according to the conventional NSF definitions of basic research, applied research, and development, one arrives at the following result:[2]

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	Defense	Non-Defense
% basic	3.2%	33.7%
% applied	11.0	35.3
% development	88.5	31.0

We see that whereas non-defense R&D is nearly 70% research, defense R&D is less than 15% research. Thus non-defense R&D accounts for 65% of federal research, whereas defense accounts for only 35% of federal research despite its dominance of federal R&D as a whole. It may also be worth pointing out that the "D" component in defense R&D is actually "development, test, and evaluation (DT&E)" and includes production runs on prototype equipment and very elaborate field and operational testing prior to acceptance of weapons systems for procurement. This may in fact constitute more than one third of the "D" part of R&D in defense; indeed the T&E part of defense would often be labeled manufacturing or pilot plant work and might not be counted as R&D at all in civilian industry.

The fact of the matter is that most of the development work connected to federal non-defense R&D is carried out in private industry with its own funds, with the exception of work in NASA and a little in DOE. Thus, for example, private R&D expenditures related to food and agriculture and to health care are approximately equal to federal expenditures for agricultural and biomedical research, respectively. Unlike the situation in defense, there is a complementarity or division of labor between federal and non-federal R&D expenditures for a given societal function.

It seems unreasonable in talking about federal support of scientific research to include the development component, especially of defense, since this is so closely tailored to final use, which has little (and declining) benefits for the civilian economy. It is very difficult to decide what to include in the committee's category "scientific research." Since the committee seems to be interested in arriving at some sort of opinion as to what is an "optimum" overall amount of federal support, this would be very sensitive to just what is included. Since research gets progressively more expensive as one moves from basic research to development the total becomes very sensitive to what parts of development and applied research are excluded.

I would infer that what the committee is interested in is primarily research that is devoted to "the advancement of knowledge" or research that is not highly specific or unique to a single social purpose such as defense but has potential applications in many areas. At the most it might be the total in the "research" category—about $17.7 B. in FY 1986; at the least it might be the amount in the "basic research" category--$8.0 B. Perhaps, one might arbitrarily count half of the "applied research" category as "scientific research" and arrive at a total of $11.9 B. as "science" in the sense that the committee intends. On the other hand, if even 10% of the $42.5 B. obligated for "development" in FY1986 were really "bootlegged" scientific research, it would amount to half of all the money spent on "basic Research."[3] Another measure might be "R&D at Colleges and Universities"—$6.55 B.—but this would leave out a good deal of academic-type research in such fields as high energy physics, astronomy, oceanography, and space science which is conducted outside of universities but participated in heavily by academics and is directed primarily at the "advancement of knowledge" in its own terms. If this were 20% of the R&D in "universities proper" the total would come to $7.8 B.—about the same as the "basic research" total. However, it does not represent the same universe of activity, since not all basic research is conducted in universities—only about 55% in fact.

The upshot of this discussion is that I am assuming that the committee is interested in something between $8 and $12 B. of "scientific research" being funded mainly, but not exclusively, in academic institutions by the federal government. The rest—somewhere between $48 B. and $52 B.—has to be judged against the particular political or social goals for which it is funded, and is dependent on the importance that the Congress and the American people attach to those goals, not the importance that they attach to science. This is not to say the money is of no benefit to science. If nothing else it provides a market for trained scientific manpower, and also helps sustain a large scientific instrumentation and equipment industry whose products are used widely in production, health care, and other operational activities—not just R&D. This is a "spillover" benefit to the economy which is of considerable importance but which, so far as I know, has never been quantified.

An important policy implication of the preceding discussion is that it is very important that many things which the public looks upon as "science" should not be counted against the science budget. In addition to large weapons development projects, items such as Apollo, the space station, or the shuttle are not really science except to the extent that they carry specific scientific payloads such as the space telescope. Such large projects should be considered on their own merits; they should not compete within the "science budget" but should affect science funding only in the same way that welfare programs, large dams, or the Amtrak deficit affect science programs. It would be a tragedy, for example, if, in a period when the overall R&D budget is shrinking, the space station were funded at the expense of the general advancement of knowledge under the illusion that it is "science." On the other hand, the superconducting supercollider (SSC) or unmanned planetary probes or a large new telescope would legitimately be considered as science, and can legitimately be required to compete on the basis of intrinsic scientific merit with other meritorious science projects, large and small. Their size should neither prejudice their funding, nor make them sacred cows.

Effects of Reductions

The committee has suggested that one way of "adding perspective to an examination of the feasibility of establishing an optimum level of federal support for science" would be to speculate on the effects of a drastic reduction in the level of federal support. If the reduction were for R&D as a whole, including the "D" in defense R&D, the demand for scientists and engineers would be significantly reduced, and immediate needs for trained manpower would be correspondingly reduced unless resources were redeployed for other social purposes such as assistance to development in the Third World or the solution of major environmental problems or some other similar purpose. One might argue in that case that the need for technical manpower with advanced training would be reduced, and therefore support for academic science and education at the graduate level could be reduced without detriment to the new national priorities. Since defense and space tend to be much more R&D-intensive than any other economic activities the demand for scientists and engineers would be reduced even if federal defense and space expenditures were redeployed with no reduction in total. There would certainly be temporary unemployment among scientists and engineers and serious financial crisis for colleges and universities, probably resulting in the closing of many institutions.

The effect of drastic reductions in the support of research would depend a great deal on how they were carried out. We know that most of the seminal advances in science result from the work of a small proportion of scientists; the distribution of citations to the published work of individual scientists is highly skewed, so that a small fraction of scientists is responsible for the overwhelming proportion of citations. The same applies in somewhat lesser measure to institutions.

One could draw two policy conclusions from the above observations. One is that we could possibly absorb a major reduction in science funding without a proportionate loss of scientific productivity if we had the political skill and will to be sufficiently selective about the individuals and institutions we continue to support. I am doubtful, however, that that would be possible within the American political system. Our system aims at a combination of fairness (equality of opportunity) and efficiency. In a crunch our system would tend to sacrifice fairness to efficiency rather than vice versa. The trade-off between fairness and efficiency becomes easier to make when resources are rising, while the two objectives tend to be more in conflict with each other when they are falling. If we gave priority to fairness and equality of access in a period of shrinking resources, I believe the result would be catastrophic not only to science, but also, with some time lag, to our economy and security as well. The reduction is scientific productivity would be much greater than the reduction in resources. But this is hard to prove because the whole system is so tightly interconnected. The real question is what the unsupported scientists and engineers would do instead. Are there other activities that would contribute greater value-added to our society? Would they become entrepreneurs, or shift to applied or developmental activities that would actually improve our economy, or would they form a cadre of disaffected intellectuals which would destabilize our political system? I do not know of any good way we can predict this.

If government were to reduce its role in the support of basic and academic science, it does not appear likely that any other institution or funding source would step in the fill the gap. Corporate funding of academic research, now about 6% of all academic R&D, is not likely to more than double at best, and would tend to be concentrated in a few fields of self-evident industrial relevance. The results of such research are simply not appropriable by the sponsors; they have the form of a "public good" from which all potential sponsors benefit whether they help pay or not. Furthermore, corporate support would likely be concentrated in a small number of institutions and in a small number of fields.

One thing does seem probable. If the problem of triage were left to the organized management of the scientific community itself, the first result would be chaotic, but in the end the scientific community would choose efficiency over equity, and the least harm would be done to scientific productivity. But the result would be the least politically palatable to the members of this Congress and to the people that elect them. The money would be distributed in what would be viewed as highly elitist manner, and within a short time I believe the process of redistribution would become politically unsustainable. Equity and fairness would win out over efficiency, and the productivity of the system would collapse.

One of the problems is that the relative advantages of scientists in the competition for funds tends to be cumulative, since research is a learning process. Those that are supported to do research become better researchers and teachers, and those that are not supported rapidly become out of date in both teaching and research. Over the years since War War II there has been a slow but steady trend towards equalization of research capabilities among institutions and regions. This has happened in part because scientists have gone out from the traditionally strong institutions and populated the faculties of the less prestigious institutions, carrying with them the standards and expectations they learned in their graduate training--possibly with some attenuation. With rapidly shrinking resources this trend would be reversed until it became politically intolerable; thereafter the deterioration of quality and standards would probably accelerate, as decisions were more and more taken out of the hands of the leaders of the scientific community.

There is another point that needs to made about declining support. Despite the high degree of skewing in the distribution of productivity among individual scientists, this does not mean that the few that are most productive are the only ones worth supporting. The scientific establishment may be more like a human pyramid, in which the productivity of the few at the top rests on the broader base and thus in some degree depends on the size of the whole pyramid. Despite the cumulative nature of scientific excellence, this is only true statistically, and there is constant process of winnowing and selection going on. There is probably a close analogy with sports. It takes a large base of people competing to produce the few top competitors.

In summary, I find it very difficult to predict the consequences of a drastic shrinkage of resources for science. The impact could undoubtedly be softened if decisions regarding allocation were left in the hands of the leaders of the scientific community, the recognized leaders of their disciplines, but the result of this would be serious politicization of the scientific establishment, and a process of decision making that would have no legitimacy within a wider political context. If the decisions were taken out of the hands of the scientists, either because they were politically unacceptable, or because the cohesion of the scientific community itself collapsed into bitter interdisciplinary warfare, the loss of productivity of science would be greater than proportional to the reduction of funds. The effects of this would not be immediate or obvious outside of the scientific community itself. Rather they would take a decade or more to become apparent in a further deterioration of American economic and general social performance. By that time the cost of reversing the trend would have mounted, and it is not clear what would happen.

Large Laboratories vs. Small Project Grants

One of the issues likely to be raised by a drastic restructuring of federal support is the balance of federal funds flowing to large multi-purpose laboratories, such as the national laboratories of the Department of Energy, as opposed to small projects grants to individual investigators in universities. At the present time total R&D support for federally owned laboratories is about three times that going to universities proper. This includes both civil service laboratories and laboratories operated under contract in federally owned facilities. However, this figure is misleading, as a large proportion of the work in these large laboratories is development. Nevertheless, the question is already being raised as to whether at least the more basic science projects in large laboratories should not be compelled to compete in the same sort of peer review process as is now applied to virtually all research in universities by academic investigators.

Certainly it is true that, as measured by publications and citations alone, academic researchers supported under the competitive grant system are more productive than scientists in large institutional laboratories supported by programmatic or block funding. One reason for this may be that complex and specialized equipment is concentrated in such laboratories, with the result that a higher proportion of the personnel is engaged in designing, updating, and servicing the experimental facilities, often with academic scientists as principal investigators. Nevertheless, there is a widespread impression that the average quality of the research scientists in most of the block-funded laboratories is lower than that in universities. For example it was found that the average citation count for chemists applying to the NSF was three times that of chemists doing research generally.

The extension of peer review to block-funded institutions would present many practical and political difficulties in my opinion, and would add greatly to the overhead load of project reviewing which is already an enormous burden to the scientific community, and may have gone well beyond what is optimal from the standpoint of the allocation of time and effort by productive scientists. Reviewers of institutional investigator projects by academic scientists would present problems of political legitimacy.

On the other hand, in some fields such as Condensed Matter Physics (CMP) some of us believe that the environment for real creativity in basic research may be better in a few of the best industrial laboratories than in even the best research universities. This is due to the fact that grant competition has become so elaborate and slow that it is increasingly difficult to modify research plans and change directions of research in response to new discoveries or new insights. I am attaching to this statement a commentary on a paper by Professor Gerald Edelman of Rockefeller

University which I prepared for a recent conference. It describes in some detail both the strengths and weaknesses of the peer review system, and makes some suggestions for alleviating some of its undesirable aspects. The implication of the observations in that paper is that it may be a mistake that we have become so exclusively dependent on the peer-reviewed competitive project grant mechanism for the support of academic research. This does not mean we should throw it out by any any means, for I feel that on balance the American version of peer review is one of the great sources of our scientific strength. But we should restore some degree of pluralism in the mechanisms of support—a pluralism that existed in the 1960's, but which has largely been squeezed out of the system in the last twenty years.

In any case, if the support of science is to be cut back it is important to examine carefully the relative productivity of science in large institutions compared with dispersed science projects in the universities and elsewhere and to consider the possibility of some rebalancing among types of institutions and mechanisms of support. When cutbacks are necessary institutions with support coming in large programmatic blocks may be in a better position to control their own priorities and rebalance their programs according to the resources available than institutions such as universities that are dependent on dispersed sponsorship from many sources. One reason for this is that in universities the logistic support of research is partly budgetarily divorced from faculty and students. They are not in a position to coordinate personnel and research funding decisions. When a research institution hires a scientist it budgets for his or her total support; when a research university hires a scientist the most it can hope to do is to provide him or her with a "hunting license" to compete for support from an external sponsor. This hunting license approach has the virtue of forcing scientists

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to compete on merit, avoiding a guarantee of support to every competent scientist, and providing some assurance against duplicative research through centralization of decision making within each discipline, but when it is used too exclusively, it can be very wasteful of the time and energy of talented people.

In my opinion a strong case can be made that a few select individuals of proven scientific creativity may be in a better position to assess priorities among fields of science and to chart the general directions of their research than any group of peers operating at arm's length; this argues for providing at least some support directly to a small minority of scientists on the basis of past performance rather than on the basis of what they promise to do 18 months from now.

Public vs. Private Knowledge

There is a general faith that a vigorous national basic research program is essential to the competitiveness of a national economy, at least in the long run. There is a danger, however, that this faith may be taken too literally, failing to appreciate that more than a strong science is needed to underpin national economic health and competitiveness. The kind of science whose support this committee is interested in is "public science," i.e. public knowledge whose rate of advance depends upon its being shared widely so that each researcher can put together bits of knowledge from many different sources. This kind of science is a "public good." That is exactly why it mostly has to be funded publicly. But just because it is a public good its use cannot be confined within political boundaries. The only advantage of havings one's basic science generated mostly close to home is that it is somewhat more likely to be used close to the point of origin than elsewhere even though freely disseminated and published. That is one reason why large corporations fund some basic research in house rather than relying entirely on federally supported academic research to provide the knowledge base needed by their engineers and applied scientists. The same applies to a nation. But if the rest of the innovation system is not configured so as to be able to rapidly exploit the results of basic research, the basic research of one nation may lead to knowledge which is appropriated by another nation—because it is superior at converting public knowledge into private, appropriable knowledge. With the dramatic improvements in transportation and communication that have occurred in the last thirty years, nationally generated knowledge is becoming more and more rapidly available to other countries. This makes it ever more important that the knowledge we are generating really does get to our own applied scientists more quickly and that they in turn become more aware of what is going on elsewhere in the world. At the present time our understanding of how public knowledge gets incorporated into technological innovation is quite rudimentary. We must not expect to realize the benefits of our own basic science unless we are also aggressive in seeking out and using the results of basic science generated by others, for no nation can any longer be self-sufficient in science. Yet the nature of the scientific enterprise is such that it is difficult to acquire the science we need from others unless we are equal to them in the generation of science. We know very little about how much science we need to be doing ourselves in order to assimilate and use the science being done by others, but it is clear that is factor that must be included in any effort to determine an "optimum."

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Profits and Prophecy:
A Partial View of American Science Near the Millennium

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It is curiosity that drives scientific effort, not profit, but in a modern age, it is profit that makes science possible. In these days of particle accelerators of ten mile diameter and large genetic engineering establishments, economies of scale operate with a vengeance. The past may have seen a gentleman of both cerebral and fiscal endowments peer at stars or animalcules while savoring the delicious rewards of the natural philosopher, but now he would need 20 well-trained postdoctoral associates to carry out his studies. What consequences does this change in scale have for the progress of science in America? Does the promised increase in technological progress prefigure a return to public belief in the general idea of progress? What are the roles of the private and public sectors in the provision of moral and financial support to an enlarging scientific enterprise?

In this article, I want to consider how notions of profit can intrude upon the scientific stance in basic research, which is that of prediction (or prophecy), and how that intrusion might alter private and public judgments of science. Then I want to describe a large scientific prophecy which if realized may revolutionize ideology to the same extent as Darwinism did in the last century. These ambitious tasks are well set in the American frame --- since World War II, America has been dominant in scientific technology, and in the last two decades she has been dominant in brain science, the main source of my prophecy. Although I shall speak in the international and ecumenical spirit that is central to modern science, I shall also speak personally as an American scientist reporting subjectively upon the changes wrought by altered attitudes in this country toward profit and prophecy in science.

Prophecy as the Origin of Profits

As the great physical chemist Van't Hoff pointed out (1), science is imagination in the service of the verifiable truth and as such it is eminently practical. Outside of the delights of recognition (2) afforded by its predictions, science has two main social effects: (i) its prophecies (predictions) can be turned to use (profits) and (ii) it can change human ideology, largely by challenging beliefs established by non-scientific means. With few exceptions, the first effect is welcomed, but the second, which is almost always the result of a large overturn in scientific theory, is usually received with mixed feelings.

It is important to notice that the source of both these effects is the same: empirical science itself. The stance of the empirical scientist is that of the detached observer or experimenter exercising imagination in the service of the verifiable truth. Clearly, this detachment implies a certain frame of mind: no non-scientific idealogy may intervene on judgment and no immediate practical ends should constrict the imaginative view. If it is carried out at all effectively in this fashion, science must inevitably make progress in both methodology and aim.

Scientific predictions and prophecies based on such progress can in some cases be converted to enormous profits. But it is not so well grasped by the general public that the technological pursuits yielding such profits are only rarely convertible into new scientific prophecies. The course of basic inquiry that leads to good scientific prediction requires an attitude toward time and money that differs greatly from that which must be adopted during the course of technological development. Applied research and development are much more expensive than basic research and they are carried out over short schedules of time at high pressure with well-understood scientific principles. Although basic research is much cheaper, it requires an unconstrained position on time insofar as its major heuristic search is for new principles or a deep revision of old ones. Once understood, these principles may of course be used for new applications.

A remarkably tortuous and unimaginable course must usually be taken before a basic scientific prophecy can be turned to profit. To illustrate this, I shall briefly discuss two profitable areas of modern biology --- molecular genetics and immunology. Although I must take some liberties with detail because this is not a technical article, in its essentials my account shall be correct. These examples should serve to illustrate the point that while the use to which a scientific principle is put can be prophesied loosely by qualified scientists, the prophecy cannot be realized as profit until a complex course of inquiry that is full of surprise is nearly completed.

This may seem obvious, but it is constantly forgotten by scientific administrators in the public sector, driven as they are by needs for definition, regulation, timeliness, and practicality. Influenced by such needs, their decisions often alter the climate required to undertake a basic inquiry and the consequences of these decisions can discourage the development of individuals more suited to that climate. The result is somewhat paradoxical because a continued growth of basic science is not only essential to applied science but it costs far less money.

Unlike applied science, basic science will not yield simply to will or to money although both are useful. My two my examples illustrate this nicely. Genetics (the science concerned with the principles of heredity) was founded in 1865 by Gregor Mendel, an obscure Austrian monk. His discoveries indicated that there were units of inheritance (now called genes) that were particulate, i.e. genes were donated to progeny by each member of a mating pair and they segregated and assorted more or less independently in the offspring. This momentous discovery was forgotten and was reestablished in 1901 by De Vries and others. The reasons for the failure to recognize Mendel's discovery have been analyzed elsewhere (3,4) and although they are not entirely irrelevant, their consideration would take us too far afield. The main point I want to emphasize here is twofold: 1) Mendel's discovery and the subsequent rediscovery of the gene provided the basis for an extraordinary liaison with Darwin's theory of evolution, the so-called modern synthesis (4). Two large generalizations---that evolution occurred by natural selection and that heredity occurred through particulate means were thereby joined and together provided the central theoretical basis of biology. Both generalizations and their adjunct predictions and prophecies were of course based on large bodies of empirical evidence. 2) Subsequently, the establishment of different genes as varieties of a chemical known as deoxyribonucleic acid or DNA made possible the development of molecular genetics and genetic engineering.

Before describing genetic engineering, it is important to note that the second great discovery (that of DNA as the genetic material) like Mendel's also did not at first receive appropriate notice (5,6). Yet this basic discovery by Avery and his colleagues at the Rockefeller Institute (6) made the Mendelian heritage potentially profitable to a degree that still cannot be fully calculated. Of course, animal and plant breeders used Mendelian principles but with Avery's discoveries and the subsequent demonstration of the structure of DNA by Watson and Crick (7), the possibility of introducing chosen genes directly from one organism to another was ready to become a reality.

It is revealing to reflect upon how long and what means it took to discover, undiscover, and rediscover these great advances. They occurred in peaks of insight over 100 years, they required great bodies of workers in different countries, and their historical course could not have been predicted. Furthermore, it took time to appreciate their significance. Even the scientific community in possession of expertise (or at least exposed to the necessary principles) lapsed in its recognition of these ideas or rather, took time to see what they meant. And the individuals concerned, Mendel, a retiring monk raising flowers, and Avery, a reclusive medical researcher working on pneumonia, were hardly driven by the direct hope of profit from their predictions.

What is the outcome for profit? It is still too early to make a precise assessment but the outlook appears remarkable. Genes (or pieces of DNA) specify the structure of the intricate molecules known as proteins that control all basic biological processes. The fact that we can introduce the DNA of humans or other species into fast-growing bacteria and produce large quantities of these proteins (hormones, drugs etc.) opens up whole vistas for the treatment of disease. Moreover, the possibility of diagnosing and altering genetic disorders emerges. A recent example is the location on the human chromosome of the gene for a heritable human disorder, called Huntington's disease, which leads inevitably to motor discoordination and death (8). The advent of molecular genetics makes it possible for individuals who carry that gene to know in advance of marriage whether the disorder will show up in progeny. I could go on with more examples but will not --- it may be more useful to give a brief account of thinking and prophecy in another basic scientific field that has also led to large profit. This example is immunology and it is one with which I have been directly involved.

Immunology arose in the late 19th century as a branch of medicine at a time when the principles of bacteriology and infectious disease were first emerging from the work of Pasteur and Koch. It was observed by various investigators that the blood (or its fluid component, the serum) of a person could prevent the growth of particular bacterium if the person from whom the blood was taken had been infected previously by that agent. In short order, it was realized that this capability was highly specific ---- it was as if the blood "recognized" different infectious bacterial types specifically.

Of course, it had been known since ancient times that survivors of a disease were often "immune" and the notion was put into practice more than a century before by Jenner, who infected people with cowpox (vaccination) in order to protect them from smallpox. But it took prophecy based on the search for the "recognizing" principle in the blood to open up immunology as a full-fledged science. At the turn of the century, von Behring showed that the blood contained substances named antibodies that were not only responsible for the recognition of foreign intruders but also for facilitating their destruction by special cells of the organism. It was realized that these substances or agents of defense were remarkably specific ---- particular antibodies recognized one infectious agent by binding to it but usually not to others.

A profound extension of this idea of specificity came when Karl Landsteiner (whose discovery of blood groups made transfusion possible) showed at The Rockefeller Institute that practically *any* organic chemical that could be made by a chemist could be specifically recognized by antibodies if that chemical were first linked to a protein and injected into a laboratory animal. This discovery allied immunology with chemistry and opened up a vast area of exploration. But the chemical nature of antibodies was still obscure --- and its determination seemed hopelessly difficult because there seemed to be so many different kinds, each of different specificity. Indeed, the body was able through these antibody molecules to recognize "self" from "not-self" to use a felicitous distinction coined by Sir MacFarlane Burnet (9). To account for their capabilities of recognition, another great scientist, Linus Pauling, made a prophecy about the key issue of specificity in 1941 that has since turned out to be wrong. He suggested that the "not-self" foreign molecules (or antigens as they were called) acted as templates around which nascent antibodies folded. The impressed shape of the antigen remained in the antibody molecules (think of cookies and a cookie-cutter) and served as the vehicle by which given antibodies recognized their specific antigens upon a second encounter.

In 1956, I was in the U.S. Army, bored by the routine chores of being surgeon to the Headquarters of European Command in Paris, a city in which I was most definitely not bored. While reading a book on immunology, I was struck by the fact that much attention was paid to antigens but not to antibodies, the molecules that were obviously the central clue to the mystery of immunity. I resolved to study the molecular structure of antibodies when I was discharged, and eventually I came to The Rockefeller University to do so, innocent of how difficult a task I had set for myself. At that time (1957) in London, the late Rodney Porter also became curious about the structure of antibodies; each of us was ignorant of the other's existence.

The obstacle to be overcome was two-fold: 1) the large size of antibodies as molecules (about 30 times larger than insulin, the chemical structure of which had just been worked out in a *tour de force* by Porter's professor, Frederick Sanger); 2) the immense heterogeneity of the antibody molecules even in one individual's blood; no two kinds were identical. Since antibodies recognized different antigens by their shape and there were so many shapes, this heterogeneity was no surprise. But in order to study structure, chemists need to have pure substances (that is, every one of the molecules must be alike in shape).

To solve this first problem, Porter and I independently set out to break the antibody in smaller pieces and each succeeded by different chemical means. In the course of work on the pieces I made (which turned out to be the natural chains of the antibody molecule) I noted that one of the chains was the same as a protein excreted in the urine of patients with a cancer of certain blood cells that make antibodies. This cancer, called multiple myeloma, invades bones and produces huge amounts of a pure protein in the blood. This protein looked in every respect like an antibody except that we could not know what it would recognize inasmuch as it did not arise by previous immunization with a foreign antigen. This discovery and those of Porter made it possible to determine the entire structure of a pure antibody protein, a task which was accomplished with my colleagues at the Rockefeller Institute in 1967. By looking at molecules from cancers of different individuals each of whom made one pure antibody protein, the chemical basis of how antibodies of different specificity differed in their shape became clear.

As it turned out, one antibody-producing cell made one kind of antibody only, a finding that corresponded with a theory proposed by Sir MacFarlane Burnet (9). In this theory, he suggested that animals with backbones already had a huge number of different antibodies each with a different shape before ever seeing the foreign antigens: encounter with a particular antigen selected the antibodies that fit best; the binding of the antigen by antibodies on a selected cell resulted in the manufacture by that cell of more antibodies with the appropriate specificity.

The two developments I have briefly described here, the determination of the structure of antibodies and Burnet's so-called clonal selection theory revolutionized and rationalized immunology as a science. Moreover, they allied it to the molecular genetics and genetic engineering whose development I have already described.

At this point, the field was ripe for profit. Antibodies were chemicals that could "recognize" other chemicals in terms of their shape. Suppose you could make one kind of antibody in a test tube of just the specificity you needed? Cesar Milstein and George Kohler attacked this question at Cambridge University using the knowledge that one cell made one kind of antibody and that some cancer cells were "immortal" i.e. could keep on dividing outside of the animal in a dish. These scientists immunized mice with a chosen antigen, took cells making one kind of antibody and fused them with a multiple myeloma (or cancer) cell which was not making antibody. The hybrid cell (called a hybridoma) kept making antibodies of that kind more or less indefinitely: like one of its parents, it was immortal producing progeny cells asexually, and like the other, it was specific in making just one kind of antibody.

As a result of this development, it became practically possible to make antibodies in the test tube that were specific in binding whatever antigen one might choose: viruses, bacteria, blood proteins etc. A whole industry in which these so-called monoclonal antibodies are made is being applied to medical diagnosis and treatment and the chemical tricks learned from the work on the structure of antibodies are being applied to modifying them in order to treat disease.

This time everyone was recognized--- all the individuals I have mentioned were awarded the Nobel Prize. But none was motivated by profit, and none took out patents on his work. All were supported by government funds and to a lesser extent by local institutional means. My work on antibodies was largely supported by a grant from the U.S. Public Health Service for the study of the emission of light by molecules (fluorescence), a grant which was not even aimed at studying antibodies. Including salaries, the budget of my laboratory from 1960-1967 did not exceed $250,000/year. My colleagues at the Rockefeller University knew vaguely about my work but pretty much left it and me alone, as they had Landsteiner and Avery before me. But this was loneliness surrounded by respect, respect shared by all for the freedom of the basic pursuit --- a major principle for encouraging the most imaginative search.

Things are different now. It is inconceivable that I would have been allowed to study the almost impossibly difficult structure of a molecule while supported by a proposal to analyze fluorescence spectra (a remotely connected field). Instead, the present policy guiding application for government grants is that you deliver the goods you promised, that you not apply for two grants that have overlapping subject matter (without running risks of slashes in their budgets), and, above all, that you stick to your last and to the name of your subject. A switch of subject matter is a costly proposition for the imaginative researcher and is promptly punished; to avoid punishment, the scientist must stop his work to write an appropriately named lengthy and specifically detailed grant proposal.

I will attempt to explore this regrettable change in a later section. At this point, I want to summarize the general characteristics guiding the pursuit of profits and prophecy in science and contrast the climates that they seem to require. It is my thesis that the separation of these climates is an essential characteristic for the maintenance of growth in each arena.

The gross characteristics of each are shown in Table 1. Without denying invention, we may say that profit in applied science is based largely on the known, is secure in principle, has a short time scale for success, is task-oriented and is fractionated into narrow channels by specialists taking up defined tasks. It is costly, for a large number of people are involved. The pursuit of profit is logically acceptable in the sense that the principles on which it is based are and must be known before it can proceed. Its key strategy is recombinational --- to take one known thing and combine it in an efficient manner with another.

Prophecy in predictive basic science is based upon a search into the unknown and it is _fundamentally_ insecure. It must not keep its eye on the clock, for it may take a long time (each of the sagas I have recounted involved blocks of time amounting to about 1/3 of each single individual's life). In basic science, the task falls upon fewer people to make the key moves and _in general_, it is relatively cheap (I leave out atom smashing, for now). It often seems bizarre and unconventional for it requires strange forays into unlikely areas, ideas which are departures from received notions, and the stubborn persistence of single-minded scholars willing to face intractable (and apparently impractical) issues. Its basic strategy is recategorical --- it challenges the accepted names of things or how they are coordinated or linked.

Three observations are worth making. First, the kinds of individuals carrying out these essential scientific activities of profit and prophecy are in general different, although very occasionally an individual can traverse from one arena to another. Second, the mores and the support structure likely to yield success for each activity are likely to be highly different. Third, it is easier to assess the profit side even from a scientific viewpoint than it is to assess the prophecy side.

Despite these distinctions, much of present-day scientific research at the basic level is governed by styles more suited to research at the applied level. This has had mixed consequences that are not easy to assess; my personal view is that it represents a turn for the worse and I shall touch upon a few current practices to support that judgment.

The Swing Toward Profit in Science

So far my account has given concrete examples only from biology. To a physicist acquainted with government interests in the military uses of nuclear power and lasers, these examples may seem quaint, for even at their most ambitious scale, genetic engineering facilities can hardly compare in cost with accelerators or super colliders having radii of miles. In this arena, one is speaking of power in big terms, power as a subject central to physics as well as political power exercised by nations as they vie in defense capability or by different states as they bid for the billions that might be brought in by having a super collider sited in their land.

Regardless of such differences of scale, both biology and physics in this country have encountered similar changes, changes that govern which individuals go into which subject and what they will find when they get there. I can speak reliably only about my own experience and shall attempt to synthesize the responses of my peers and junior colleagues to changes in research sociology that have occurred in the last decade. The anecdotal nature of this account may vitiate it to some degree; nonetheless, I believe that it is symptomatic of large sociological changes in the support of American science.

It is obvious that the scale of an individual research operation is now much larger than it used to be. If the annual budget of my laboratory in the days of antibody structure determination was at most 250,000 dollars, it is now well in excess of one million dollars. The reasons have largely to do with inflation and mounting salary requirements of trained personnel in an urban setting, with the labor intensive quality of the new technology, and with the material costs of that technology. Moreover, much of the activity in modern biology is dedicated to a vast information gathering effort: there are at least 200,000 human genes and the structure of each is in principle determinable. Typical of most human effort, however, the search at any one time is usually focused upon just a few examples by a number of laboratories and there is fierce competition to come in with a first description.

A combination of these factors with changes in the structure of government funding has tended to alter the atmosphere required for prophecy as well as to make that atmosphere more closely similar to that of applied research institutions in industry. The basic-scientific entrepreneur has emerged as a definite type. He is a man of shrewdness, knowledgeable but not necessarily skilled in his subject area, capable of enlisting support either from the government system or from business (with rights of first refusal guaranteed to the donor of research funds), capable of managing up to 100 workers, active and visible on international scenes, and an author on large numbers of papers each with large numbers of co-authors. Despite these activities, he mixes freely with the more old fashioned types and the telephone is one of his main scientific instruments.

A somewhat different but related phenomenon is the emergence of what I shall call the premature professor, a young person out of Ph.D. training for 3 years, who is searching for an Assistant Professorial appointment, and for a grant of sizeable dimensions that he will exercise by fulfilling exactly what he promised to do in his grant application. While he generally will aim to emulate the basic science entrepreneur, at least he is in his laboratory fulfilling the promises of his specialist proposal. His starting budget (for a biology laboratory) can be as much as 150,000-200,000 dollars per year. He is generally highly skilled and active but is in no way inclined to dream or be obsessional about a scientific question in a serious way. His goals are to achieve specialist recognition and a stable career. In general, if one such individual can be found in a university in a particular area such as immunology, there are ten similar individuals to be found elsewhere, all fairly accomplished. The premature professor does not cross subject boundaries readily or often. His concern (as it must be, given the nature of the stringent system for reviewing his grant proposals) is apparent productivity --- not one paper of depth to be recognized as a breakthrough ten years hence, but ten papers a year. Speed, certainty, and increasing laboratory size are all reasonable intermediate range goals for the premature professor. These goals are set prior to his emergence, and it is probably the case that most such young persons have few alternatives but to pursue them for survivals' sake.

What is gained and what is lost from the emergence on the scene of such scientists? Clearly, much has been gained in the areas of methodological improvement and sheer accumulation of fact. The rate of emergence of new facts and of small answers is greatly increased, as any one perusing the key specialist journals may witness. Science in this mode has achieved statistical success --there are more facts, established more quickly and more quickly perceived. In this respect, basic research reflects (with some social lag) what has already occurred in other parts of our society. On the average, assessable performance has improved while individual depth of performance (if it is assessable at all) is not as great. There is a tighter governance of money spent and greater accountability. Regulatory provisions of funding agencies pervade, both at the level of the individual and his home institution.

It is more difficult to determine what has been lost because of these changes in the practice of basic science for several reasons: depth of performance is a qualitative matter affecting individuals and very little time has passed. To see whether the number of fundamental discoveries will be compromised by such changes, we may have to wait 35 years or at least one-third of a lifetime (a number equivalent to the time necessary to do something in basic science as I indicated in the historical anecdotes). What is clear, at least in my experience, is that the complaints and the fears of the individual scientists have changed and my impression is that the shift indicates loss. Where the prevailing complaints were previously against the difficulty of the material (i.e. the impenetrability of a scientific question) or against the unfeeling bureaucracy of University administrations, they now tend to be against the tendentiousness of peer group review or the failure to get more money.

It is clear from what I have said that only government support can handle the increased scale of most modern scientific efforts, however valiantly even rich private foundations try to lend their help. The change wrought by economies of scale is clear: with the loss of private support as a major means, there is loss of a characteristically American pluralism, a source of diversity which handled in a non-uniform way the mismatch between intent and accomplishment that must occur in basic science. While this non-governmental support system was non-uniform in its standards and it was never enough, it did a creditable job in handling a diversity of research approaches.

With increasing growth and inflation, even government support now threatens curtailment and at this point, business has stepped in with an eye to profit. In biology, this has meant that certain individual scientists have received large guaranteed sums for basic research in exchange for rights of first refusal should something practical emerge. By and large, this is a reasonably loose end liberal arrangement, but it is not without the implication of a defined target that goes against the grain of basic research. Nonetheless, a number of universities under monetary pressure, as well as individual researchers seeking relief from the tense bureaucracy of federal funding, have chosen this route.

The present picture is a shifting and eclectic one composed of mixtures of new and old customs and it is not easy to predict how much gain will accrue to basic science in America. But if there is any generality in my two anecdotes about the style of fundamental biological research, we can see sufficiently well to issue some warnings. First, the support of fact-gathering and methodologically oriented research driven by industry support with short-term goals may select competitively against individuals and climates that allow the pursuit of deep questions. Certainly the atmospheres of many biological laboratories are at present not those of dreaming, or of refining questions, or even of questioning old answers. Instead, the rewards are for action and for getting answers fast.

My second warning is explicit: universities or institutes will lose sight of long range goals and individuals of great creative gift. Imaginative persons who do not enjoy endless competition will be selected against; individuals with managerial skills will be selected for. Over some intermediate range of time, research styles of proven value may be lost and a tradition may be broken. That tradition might be summarized in the words of an accomplished scientist and mathematician (10), the late Mark Kac: "Universities should be that part of our society dedicated to fifty years hence." If things go on as they are in basic science laboratories, the goals set will be at best for five years hence, the maximal period of a government grant.

A third warning has to do with the individual discontent engendered by the paradigm of statistical success. In the aggregate, there seems to be more output of significant scientific facts but there is less contentment with the individual circumstances effected by unending competition and the need to manipulate the support system. This situation does not differ from other situations and arenas of society in which the paradigm of statistical success applies. In medicine, diagnosis rates are on the average better than 20 years ago, but the independent role of the individual physician (and I might add, the individual patient) is reduced. In basic research, the effect of the statistical paradigm will be to select against the most imaginative long-range projects. Lest this seem a small price to pay, let me suggest that it has not been assessed whether the present apparent success of the statistical paradigm does not feed in large part off the achievement of its more inefficient precursors. If this is true in science, the maintenance of the large competitive establishments and the competitive exclusion of dreamers may result in future impoverishment.

My fourth warning is against satisfaction with the merely combinatorial aspects of contemporary research efforts as distinguished from the recategorical aspects of truly profound basic research (Table 1). I shall call this satisfaction the Magister Ludi effect, after Hesse's utopian (or cacatopian) novel Das Glasperlenspiel (11). The Magister Ludi or Master of the Games could recombine knowledge but not create it. The fact is that the number of possible recombinations of current knowledge is effectively endless. As in a modern urban environment in which the recombination of just 6 or 7 technological inventions can fill everyday life with an overwhelming density of tasks, so in science one may get on forever with endless fretwork. But the key goal of basic research is to develop new insights, new questions, and new categories. Although some recombination is required, a visit to unknown jungle suburbs is mandatory. The inculcation of the questioning view --- Why should we stay here? Why not overthrow the subject? --- is under threat. Few scholar scientists are left and if their absolute number was always small, then their relative number is today sharply diminished. People must be reminded that knowledge is neither static nor just recombinatorial. It is dynamic and context-dependent and new discoveries are needed for the tasks of refurbishing and reshaping the meaning of old knowledge.

All of these qualitative and subjective remarks would be worthless if they did not generate more than a querulous account by one individual of the consequences of the present system of research sponsorship in America. I shall propose three positive suggestions. The first focuses on the practical dilemmas of funding a search into the unknown, an activity that I shall call the capitalism of curiosity. The second is a proposal in which I suggest that the most important defense to be taken by science against its own large movements is moratorium --- the creation of small places or scientific monasteries conserving old styles of inquiry. The third is nothing less than a millennial prophecy --- that, despite its present difficulties, American science (and science in general) will see another conceptual revolution before the turn of the century. That revolution is in the field of neurobiology or brain science and the message is optimistic: both science and society are likely to be transformed for the better as a result. I shall consider each of these suggestions in separate sections.

The Capitalism of Curiosity

Basic research is necessarily an inefficient process. It is not subject to ordinary rules of management and stewardship. Instead, it develops rich and unpredictable stores of facts and ideas that comprise a kind of research capital, which can be spent later for practical ends such as the cure of disease. But, like other capital, attempts to spend it before it is accumulated can be hazardous. Although many people think that the line from curiosity to application is a straight one, my anecdotes have shown that this is not the case. The fact is that research capitalism or the capitalism of curiosity follows a quirky and difficult route, in no way subject to the ordinary rules of good business practice.

One reason that this is so is that basic research is a reflection of a kind of play. This characteristic tends to be obscured by the technical and planning aspects of research that scientists must make one of their major concerns. Nonetheless, I believe that the spirit of play underlies most basic research efforts (10). Beyond a certain point you can't make play efficient without ruining it. If that is true, the imposing question becomes: Why pay scientists to play? My reply is that there is no other way to assure that proper climate in which fundamental discoveries about our world can be made.

The task of basic research is to discover and describe significant new features of that world under the guidance of a surmise, hypothesis, or theory.

This is frequently misunderstood. Often I've had graduate students ask in the middle of an experiment, "Why are we doing this? Can't we just go to the library and look it up?" That this question would come from students of science might seem astonishing, but it indicates a point of view that is common enough even in specialized precincts. The answer is, one can't look it up -- one guesses and one tries. The question reflects misconceptions about the scientific method: whatever it is, this method is not a formula to calculate discoveries. It is something to tidy up one's fumbling and guesses after one has finally found some apparently coordinated facts or clues to the solution of a problem.

In basic research, we often don't know the connection between the results of our studies and their practical applications. But as we have seen, there is one reassuring historical point: almost always, apparently inefficient basic research pays off handsomely, efficiently, and in unpredictable ways. It is obvious that a new vaccine the use of which prevents the spread of a major incapacitating illness is a vital development in both humane and economic terms. What are not so obvious are the humane and economic benefits of a basic discovery that organizes a field, opens new vistas or makes development of a vaccine possible. What is the calculable dollar-and-cents value of the structure of DNA?

To take a more personal example illustrating this last point, I am always embarrassed when reporters ask me to explain the practical benefits of my own work on antibody structure. That work has cured no disease, but it has helped to reorganize our thinking about the causes, diagnosis and therapy of diseases. For example, now that we know the structure of the antibody molecule, it can be cut up in various ways and its properties altered in order to treat people more efficiently, in ways that the body itself has not yet managed to contrive through evolution. In other words, this kind of knowledge is basic research capital which is ready to be invested in useful and humane projects. For the most part, the accumulation of such research capital does not proceed in a straight line nor does it begin with a narrow end-result in mind.

Having made the point that there is a necessary inefficiency in accumulating research capital, we may ask the question about how such inefficiency can be supported by real capital. The present means relies upon a system of peer-group review, calling upon scientists both to determine the merits of other scientists' proposals and to criticize their proposed budgets. Such reviewers interact with a trained bureaucracy in institutions such as The National Institutes of Health (NIH) and the National Science Foundation (NSF). This system has many merits but it seems to me that it may be in need of larger insights. One is that more recognition must be given to the fact that the capitalism of curiosity is a curious form of capitalism indeed -- one which lacks the usual buffers of space, time, or money that a capitalist or a businessman would employ to put his funds to the best use.

Certainly, there are features of research funding that no sane capitalist would tolerate in his business. Basic research funds usually are given via grants in fixed periods after proposals have been reviewed by scientific peers and agency officials, and in the case of government agencies, only after the funds have been appropriated by the Congress. The plans proposed by large agencies often fail to recognize sufficiently that a fairly large amount of basic research capital must be accumulated before societal application is possible. In the absence of this capital, direct mission-oriented pushes or large task-forces almost always fail. They are not even justifiable as propaganda to persuade Senators who vote the budget, for they most certainly will lead to disappointment and skepticism. Government funds usually must be given according to strict budgets for defined periods of time, and this is all well and good. But a close scrutiny of the creative process indicates that, if there are any untoward developments in a project, or if it needs change during the research period, the basic scientist has recourse to only one method — he must reapply over time periods that usually are too long to do him any good or to meet the immediate economic crisis that he faces. Even if an official at an institution such as the NIH has understood what the problem is, he will have a very hard time rustling up sufficient contingency funds to tide the researcher over. The needs for accountability, the political process, and large increases in the number of applications all are in some sense in conflict with the needs of the established researcher.

As salaries and fringe benefits increase over the time period of a grant, the total proportion of the budget that they occupy is huge and the unencumbered funds available to the researcher represent a diminishing and sometimes minuscule part of the budget. Project costs and demands cannot easily be budgeted ahead in any realistic fashion. On the other side, one must consider the labor-intensive and distracting task of grant renewal as well as delays in the grant decision-making process. If a scientist has three or four grants supported by different agencies with different starting and ending periods and one grant ends, a remarkable juggling act is essential in order to keep a constant flow of money coming to the laboratory. This requires the cooperation of officials in the various agencies as well as the cooperation of officials in a home institution such as a University. Although a grant may be awarded in principle, a grant-decision delay means that the money simply is not there, and some way of buffering the absence of that money must be provided.

In addition, there are other long-range needs that create additional major stresses for institutions. One of these is the increasing cost of setting up young researchers in a new research venture -- beginning Assistant Professors, for example. Everybody in the research enterprise knows that young brains are the ones that we can count on for new developments and imagination in a variety of fields. But it no longer is possible to have every kind of field represented in every good institution. There must be some fractionation of fields, an event which can influence both grant making decisions and local administrative support.

In the face of these difficulties, the question one is tempted to ask is, "Given such short- and long-range needs, would a businessman run his company without some form of buffering provided by equity, investment capital, and bank loans?" I think it unlikely, and would therefore suggest that perhaps the time has come to consider the idea of a research bank, a place in which government funds are placed to buffer the various situations and contingencies that arise during a research project. Such a bank could serve to meet a number of contingencies that confront the researcher in the course of his basic research. Of course, this proposal of a research bank is not sufficiently detailed, and there is a conceptual problem: basic research provides no direct profit and no obvious tangible product. Nonetheless, as I have tried to show, science has an invisible product with enormous implications for our economy. Indeed, some economists have estimated that over 30 percent of the economic development in this country since World War II can be related in a more or less direct way to the basic research accomplished in that period.

The idea of a research bank may not be practical in the end because of the political difficulties that it might engender. Moreover, private support provided by local institutions already serve many of the functions of a research bank. It has always been a tradition of good research institutions to provide some of the buffers that I have been talking about, but it is a hard tradition to maintain in the face of mounting costs. It is time to reinstate the support of excellent local institutions at the basic level by private sources and foundations without concern for the same notions of democracy that must be heeded in disbursing government funds. This will require an aggressive campaign to educate the public and to inspire private foundations that no pretext of practicality or democracy is required. Presenting university needs as equivalent to those of a research bank may make attractive good sense to businessmen.

The Scientific Monastery

Nisbet (12) has pointed out that, in history, the ruling mode is either religious, political, revolutionary, or monastic. So far, I have suggested that basic research originates in imagination and curiosity, that it cannot be predicted, that it takes a long time, and that its major progress usually turns unpredictably around the work of a few individuals trained in a certain 'tradition.' At the same time, exploiting the practical benefits of this research in applied work requires prediction from known principle, short-term rewards, a large number of people, and much money. As progress has occurred, these two modes have become mixed and this has generated many ambiguities about the magnitude, style, criteria of success, and sources of support in the scientific enterprise.

We now face a new dilemma -- basic research is beginning to cost more money. This eventuality means that, if the original view ruling basic research was quasi-religious (the university as a place for 50 years hence with the researcher-professor as the inculcator of a tradition, guiding searches for the right question) the view now ruling the roost is political. That is to say, many or most of the discussions among scientists now concern who gives the money, who gets it, and how it is to be spent. Is there an alterative to this view?

Science cannot by its very nature adopt a revolutionary mode in the sphere of politics. And it is not likely that the present administrators of big science or of business-supported science will change their criteria, for much that is being done is good for the country, for technology, for business, and for science. If we accept these conclusions, how do we preserve what we also know to be good in the scientific tradition about sources of individual creation and the teaching of styles related to deep questions? I submit here the suggestion of the scientific monastery, a small privately supported institution for refining questions. Science, of course, is the art of the soluble (13), an exercise in answers. But the answers come from framing the right question in a nourishing and supportive environment.

As Director of The Neurosciences Institute, an institution dedicated to providing such an environment, I run the risk of the accusation of special pleading by describing its activities. Rather, I hope that it may be taken as a sign of special fervor if I describe the activities of this Institution as one kind (not the only imaginable kind) of scientific monastery. In its heyday (and to a certain extent at present), the Institute for Advanced Study in Princeton was another kind. The Neurosciences Institute (NSI) was founded 4 years ago as part of the Neurosciences Research Program (NRP), a voluntary association of scholars in the brain sciences begun in 1962 at the Massachusetts Institute of Technology. At any one time, the NRP is composed of 32-36 scientists who are centrally interested in brain science. Their original purpose was to develop increased communication across the boundaries of the ten or so subdisciplines in neuroscience by arranging for frequent meetings of small groups and an occasional grand summary across all fields. The Neurosciences Institute (NSI) was begun in 1982 to help an individual basic scientist refine a main question of interest in his field and to formulate experimental or theoretical problems that might require meeting with specialists even from remote fields.

The NSI is housed at The Rockefeller University but is separate from it and supported by a not-for-profit foundation, The Neurosciences Research Foundation. The rules of the NSI are few: there is no tenure; people may come or go on any schedule that pleases them or their home institution; no more than 12 people may be at the NSI at one time; and no formal scientific meetings are allowed. Instead, each Fellow may arrange a conference of up to 8 people around any issue he pleases. Fellows are allowed to appoint one Junior Fellow without further review (a Junior Fellow being one with Assistant Professorial or Postdoctoral rank); the chosen Junior Fellow may come to the NSI at the same or a different time as his designator. Junior Fellows are also appointed independently by NRP associates. In its three years of active existence, the NSI has been host to more than 500 scientists from 130 institutions and 17 countries.

One of the most successful enterprises of the NSI is the so-called Summer Atelier run entirely by Junior Fellows. Each summer, these Fellows invite up to twenty persons (from different countries and in whatever field they deem suitable) to join them for up to 5 weeks to consider the refinement of questions of mutual interest. Several scientific collaborations have begun as a result of these encounters; young scientists in these collaborations may use the resources of the NSI throughout the year without elaborate application procedures. These resources include a large computer, a specialized library, a staff organization trained to expedite conferences, an editorial staff, and, above all, interesting people. Practically all Fellows and Junior Fellows report alleviation from bureaucratic despotism as well as a burst of creative activity during their stay. There is every evidence that a mutual scholarly reinforcement takes place at the NSI. At the same time, there is no competition with home institutions: individuals return to these institutions to carry out the research planned at the NSI.

In a field such as neuroscience which consists of many subdisciplines, it is perhaps particularly easy to justify the existence of an NSI. Nonetheless, one may envision many fields in which a similar institution might be useful to help counteract the competitive, specialized, relatively bureaucratic tendencies of present large-scale research. Such institutions must, of course, be privately supported by enlightened individuals or by Foundations, for they could hardly deliver the kind of product demanded perforce by Federal agencies. Nonetheless, they are places for restoring belief in the obligate refinement of scientific questions and in pluralistic modes of behavior, of support, and of scientific exchange.

In this sense, such scientific monasteries are a return to the style of small science based on the individual without taking any adversarial stance against the necessary or inevitable trends of big science. In other words, while institutes such as NSI are not practical, they are useful in sustaining the tradition of the imaginative individual scientific scholar in parallel with big science. At their budget levels (0.5-1 million dollars a year), they are the least costly alternative to the large political modes of carrying on specialist science. In dealing with problems of scale by self-selection and mutual education, they bring historically proven advantages to bear upon the problem of preserving and encouraging imagination in basic science. In this sense, they are practical.

This issue of practical progress and its value brings me to the final part of this essay. In this part, I want to ask about the ideological consequences, not just the practical consequences, of scientific advance. This seems appropriate 15 years before the year 2000, as long a period as can be sensibly forecast in scientific development.

The Observer Observed: A Millennial Prophecy

It is perhaps time to return in this essay to the true end of science: to enlarge our view of ourselves and of nature. Science is embedded in a particular society at a given time in history and to some extent it is subject to the beliefs, trends, and values of that society, even to its whims. A review of the history of the support of American science from the beginning of World War II to the present would amply sustain that statement. At the same time, however, science alters the acceptance of ideology (at least by contradiction of promulgated false beliefs). While it would be absurd to think that science can predict historical events, scientific descriptions and products set important boundary conditions determining the frames within which events unfold and beliefs are shared. The development of automation, nuclear power, computer technology, aeronautics, and the exploration of outer space have altered the pattern of our lives. The very interdependence of such systems has put us at hazard: in a psychic equivalent of Malthus' law, if the fraction of psychotics or fanatics in a society has grown no greater, the possibility that one or a few might cause irreparable damage has grown exponentially. At the same time, the advance of biology and of medical research has brought an extraordinary set of benefits to mankind without national restrictions.

Science, above all science in America, represents the very best principles of internationalism. This internationalism is, of course, obligatory in the pursuit of basic science, but even in the applied sphere, this country has been generous in opening opportunities for others, whether it be the support of physicists during the 30's by the Rockefeller Foundation, or the export of medical advances by that and other institutions in the 50's.

Internationalism in science is one of the most impressive realizations of the idea of progress. This idea, which sweeps through Western history over 26 centuries from Hesiod through St. Augustine, and from the Reformation through the post-Newtonian age, has two main ingredients as Nisbet (14) has pointed out. The first is the gradual but definite increase in human knowledge. The second is the continued improvement of the moral state of humanity. Both of these elements can be discerned in a series of developments stemming from medical research, from the humane pragmatism of pre-scientific medicine to the diagnostic and therapeutic triumphs of modern scientific medicine.

As we approach the millennium, can we say that this progressivism will continue without limit, or that the successful practical applications of scientific medicine will reinforce the general belief in the idea of progress? I think that this is by no means guaranteed ---- at the present moment, we see an increasing bureaucratization of our technology and a waning of public confidence in science. Even the flow of practical advances of medicine to countries in need in the best spirit of internationalism will not, I believe, bring about a reversal in this trend. The reason is that, as important as practical medical scientific change is to individuals and countries, it has no direct effect on human belief.

There is, however, in the offing another development in basic medical research that promises to bring about as large a revolution in the understanding of our nature as has been achieved in the Copernician, Darwinian and Einsteinian revolutions (15). I refer to the rapidly increasing understanding of the biological bases of the workings of the human brain. This is, simply put, like no other scientific adventure yet embarked upon.

Let me defend this view by a few summary statements. First, the brain is a special organ --- it is at the center of human nature and human concern and to understand it as we understand a kidney would therefore not merely imply another technological triumph but rather an epistemological revolution. Second, the brain is not like its works or products. There is, for example, no evidence whatsoever that the biological operation of the brain will be adequately described by the principle of physics alone as a logic machine by analogy to a computer (16). Third, these two statements are presently not generally recognized as having major revolutionary implications for science or society. Instead, the prevailing view of science is based on the traditions of physics, that is upon simple systems utilized by societies for the transmission of power. From the view point of basic biology however, it appears that the operation of the brain resembles complex systems more akin to those of evolution, i.e. the operations of the brain are exercises in variation and selection (16,17). While we are far from understanding fully the mechanisms of these operations, progress is rapid and by the millennium, I believe we can expect a revolution.

What is it we will know when we understand the mechanisms of brain operation to the same degree that we presently understand the kidney or other organs? I suspect that we will regard the possessors of human brains once more as precious sources of individuality and variability and not as computer-like entities to be made more efficient. I believe that we will also have a richer understanding of the Lamarckian principles of social transmission (18) that, in a species like ours, complement the Darwinian principles of genetic transmission.

Most important of all, we may expect these developments to give a large boost to the idea of progress. The idea of progress is, after all, an idea concerning the workings of the human mind and that mind arises in the workings of the brain. It has only barely been envisioned what life will be like when we have an understanding of the material basis of what it is to be a human being. Recent advances have already made it conceivable that we will soon understand the molecular bases of anatomy with a great increase in understanding higher brain functions and physiology of the brain. It is, of course, more adventurous to predict that all of this will have a positive effect on the idea of progress. If we admit, however, that the brain is at least the necessary base of human definition, knowledge of the brain is sacred knowledge. And without implying mysticism, it is the notion of the sacred that has driven the idea of progress in society (14).

Progress is not just power, it is insight --- we are on the threshold in medical research of the largest single insight imaginable into our own nature. It would be tragic if we shrink from it or if, in America, we failed to lend it support. To do as we have always done to support medical research in America will not be enough. It is now necessary to consider how that support should be given in order to safeguard the entire scientific enterprise as well as the public interest. I believe that the questions raised here about our current modes of support are worthy of continuing debate and that simple practicality and progressivism are not sufficient to the case posed by the exploration of the brain. The engagement of public and private sectors in America in the support of brain science will require new modes of support and great imagination. The scientific exploration of the material basis of inner space has begun, and in it lies a new hope for our view of ourselves and of our future.

TABLE 1

Characteristics of Applied and Basic Research

	Applied Science	Basic Science
Scientific Principle	Understood	Object of search
Time Scale	Short to medium range (1-5 yrs)	Long range (5-100 yrs)
Cost	Great	Relatively small
Workers	Highly specialized; large numbers	Cross-disciplinary; smaller numbers
Strategy	Recombinational; inventive	Recategorical; imaginative

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COMMENTS ON GERALD EDELMAN'S PAPER

The principal argument of Professor Edelman's provocative paper is that there has been a subtle change in the sociology of American academic science which may pose a threat to its future vitality and productivity. Drawing primarily on the experience of his own fields of interest of molecular biology, immunology, and brain science, he suggests that the dependence of academic science on the federal government, the increased formalization and bureaucratization of peer review, and the intense competition for funding are combining to erode the conceptual depth and the boldness of research goals which have been the hallmarks of postwar American science, and which have made it the envy of the world. While he himself makes no claim to have proved his thesis, his paper is disturbing, and I am inclined to agree with its main thrust.

What I would like to do first by way of comment is to examine this thesis from a rather different perspective, based on my own observations of a field of research with which I am familiar through my own background--condensed matter physics (CMP).

Although it has had a very different political and sociological history than the fields which Edelman discusses, CMP also has many features in common with them. Despite the rapid growth of its requirements for elaborate instrumentation and for teamwork and interdisciplinary collaboration, it would still be for the most part classified as a "little science," in contrast to fields like nuclear and particle physics or astronomy which are clearly "big science," almost wholly dependent on a small number of expensive one-of-a-kind facilities. It is heavily experimental, dependent on a very diverse array of experimental techniques (but accessible to most workers in the field in their own laboratories), and a close interplay between theory and experiment, with most experimenters being their own theoreticians. There is a close interrelationship between the science and its potential applications, with laboratory techniques readily translated into commercial instrumentation and operational procedures, albeit with a considerable additional investment and a different style of funding and management. Unlike the situation in Edelman's fields, however, the center of gravity of this science now largely resides in industry, or rather in a very small number of large industrial laboratories, whose environment and sociology comes closer to reproducing the ideal which Edelman cites than is now the case for universities. It is this fact which I think provides additional evidence for just the point that Edelman is making. A few great industrial laboratories are providing an environment that is more conducive to the kind of patient creativity and conceptual depth that Edelman considers necessary to truly great scholarship than the federally funded universities are providing—even the best of them. Two good, but by no means unique, examples of such institutions are the Bell Laboratories and IBM corporate research.

Of course the kind of research we are talking about comprises less than five percent of the total research and development activity of these organizations, whose combined total R&D budgets exceed the total amount of money the federal government invests in academic research. The rest of the R&D activity of these giant organizations conforms much more to the description of applied research that Edelman gives in his paper, although, at least for CMP, I think he overdraws the contrast between basic and applied activity, which is maintained deliberately "fuzzy" in these organizations. This intentional fuzziness may in fact be essential to the creativity of both the basic and applied sides of the house.

When one of these laboratories decides to employ a fresh PhD out of a university to do basic research, it searches out the most creative young scientists it can identify and tries to provide them with a total environment in which their talents can flower. When a university hires a young assistant professor in CMP, it is equally careful to identify the best, but, unlike the industrial laboratory, what it provides the young scientist with is a guaranteed salary, supportive and stimulating colleagues, and a hunting license to seek funding and equipment from federal or other sponsors in order to run a viable modern laboratory. In contrast, for the industrial laboratory, the very act of hiring the scientist entails an automatic commitment to provide him or her with all the logistic support (within reason) needed to carry out an effective research program. Aside from the fact that the general broad area of research is deemed to be in the long range interests of the company's business, very few constraints are placed on the actual choice of projects, and there is no pressure on the scientist to identify in advance the possible applications of his or her work, since the R&D manager has made a prior decision that the general field is of potential applicability, and assumes the burden of responsibility for defending that decision to the top corporate management. Top management in turn does not demand accountability for that decision on a care by case basis; it demands only that the young scientist do first class research as judged primarily by his/her colleagues within the laboratory and secondarily by his/her peers in the same or related fields in other institutions. If management feels it cannot afford the resources necessary to fully support the young scientist, it does not create an opening in the first place. Professional man/years is the unit of support. Evaluation of performance is long-term, based on interactions with colleagues and peers, and the individual is given considerable discretion in the use of the resources made available to him/her.

Recently I had occasion to review the results of a faculty search for a young tenure professor in the field of CMP in a leading university. The net was cast very widely; there was an intermediate list of about a dozen candidates and a short list of about five, drawn from all over the world. What was striking about both the intermediate and the short lists was the predominance of candidates from the two industrial labs I mentioned. Furthermore, in the case of the few academic candidates that remained on the short lists it was striking that almost all the recommenders felt it necessary in assessing their accomplishments to make some allowance for the disadvantages of their having worked in a university . In the case of industrial candidates several recommenders speculated on whether they had the entrepreneurial and managerial talents to be able to move out of the much more "academic" industrial environment and survive in the grantmanship battle of a research university. Several otherwise attractive candidates of close to Nobel Prize stature had eventually been winnowed out by the search committee because it was predicted that their achievements would be considerably more modest in the less supportive environment of the university than they had been in the industrial laboratory from which they came. In other words it appeared that the two industrial laboratories in question were selecting more effectively in favor of Edelman's "certain research styles of proven value" than were the universities.

Thus I think the above anecdote provides disturbing support for Gerald Edelman's thesis. When an institution committed to maximizing scientific creativity and productivity, with full control of its own resources, chooses to develop an environment so different from that of the university I think there is something wrong in the university, and the results achieved seem to confirm this.

Of course, the academic scientist has many distracting obligations to which the scientist in the very best industrial labs is not subject, and so the comparison may not be entirely fair; yet one of the arguments for the academic environment has always been the stimulating effect of teaching on the generation of research ideas and particularly the posing of old fundamental questions in new ways, which Edelman identifies as one of the most important hallmarks of truly path-breaking research. Thus the question is whether these indubitable advantages of the academic environment are now significantly outweighed by the "selectivity against certain research styles of proven value" and the excessively competitive environment in which academic researchers have to operate--particularly the younger researchers. Does the atmosphere of the research university--largely the product of the dependence on federal funding--disciminate against the quiet but deep scholar who is uncomforttable with the continual necessity of justifying his work to others and laying out the path of discovery in advance in great detail.

I am glad that Edelman, in identifying a disturbing problem, does not advocate throwing out the baby with the bath, like some recent critics of peer review. I believe, and I think he would agree, that the principle of competitive peer review as we have developed it in the United States is one of the great sources of strength of American science compared to that in the rest of the world. For example, close observers of the Soviet Union, which has more scientists per unit of population than the U.S., have often called attention to the remarkably low productivity of the Soviet research system in experimental science, although they are frequently at the forefront in pure mathematics and the more theoretical sciences. This lag is almost always attributed primarily to the lack of effective peer review in the Soviet system, combined with overspecialization and the lack of an adequate industrial infrastructure for designing and producing reliable instrumentation and laboratory supplies. From my own considerable first hand experience with the Soviet system, I agree with this interpretation.

Yet I think one can make a case that in recent years the U.S. may have gone overboard on the virtues of competition both in science and economics. We have operated on the theory that if some competition is good, more must be better, and that if resources are constrained the best way way to ration them is through unrestrained competition. Competition has social costs which somehow or other inevitably end by being incorporated into the overhead costs of everything else in the society, and that is as true in science as in the economy. In basic science one of these costs is the amount of shear intellectual effort and creativity that goes into writing and evaluating research proposals. Not all of this is wasted by any means; it leads to better planning on the part of researchers, and involvement of active researchers in the peer review of research proposals provides a kind of automatic mechanism for coordinating research plans among widely dispersed investigators which undoubtedly saves much unnecessary duplication, thereby increasing the efficiency of the overall research system. I doubt if we could operate such a highly dispersed academic research system as we have in this country without some form of centralized peer review. But there must be a threshold beyond which there are rapidly diminishing returns, and I think there is little question but that we have passed that threshold in many of the most fundamental and competitive fields of basic science, as Edelman suggests.

Edelman shrewdly points out that the project grant system is designed to maximize the statistical success of federal grant money. It is a system based on expectation values of results, which tends to discriminate against extremes and outliers--in short against the idiosyncratic.

There is ample research data on scientific productivity to show that there is a high statistical correlation between high productivity in science as judged by such measures as paper counts and citation indices and high achievement as judged more qualitatively by peer evaluations and professional recognition. Eugene Garfield, President of the Institute for Scientific Information, has been quite successful, for example, in predicting future Nobel prizes from citation counts of the authors of scientific articles. Undoubtedly the peer review system has been very successful in eliminating much unproductive or plain crackpot research, but it may have a tendency to eliminate both very good and very bad research. Like almost all systems based on consensus judgments, it is not very good at balancing low probability of success--especially early success--against potentially enormous intellectual gains. Having pluralistic sources of support helps this somewhat, in that it reduces the adverse consequences of single errors of judgment; pluralism is also identified as one of the strengths of the U.S. system. But it may be, as Edelman's discussion implies, that pluralism in the modes and styles of support may be more important than merely plurality of sponsors, especially as improved policy coordination tends gradually to make selection mechanisms more uniform across federal agencies, and even between government and private agencies.

Diversity of support mechanisms has greatly declined in recent years. In 1963, for example, only about 70% of the money flowing to universities which contributed to their research effort was actually in the form of direct research support for projects or laboratories; about 16% was for fellowships, training grants, career investigator awards, and postdoctoral fellowships, awards to individuals based on scientific promise; another 10% was for institutional grants, and 6% was for construction of facilities. Today these other modes of support have all but disappeared, leaving only competitive research grants.

The several political attacks on peer review as "an old boy network" which have been mounted in the last fifteen years have led to greater and greater formalization of the review process. Peer review has always had two not always entirely compatible purposes: efficiency--the most scientific output per dollar of research investment--and fairness--an equal chance for all claimants on the system. Since the system is much more vulnerable to political attack on the fairness criterion, however, fairness has tended to be more and more stressed as time goes on at the expense of efficiency in the achievement of important scientific results. The review process takes longer and longer; more and more detailed justification is required, and research plans are required to be specified longer and longer ahead of the actual conduct of the research. Since grants are presumed to be awarded on the basis of what the investigator has proposed to do, Congress has insisted in the interests of fairness that investigators must be required to deliver what they said they would deliver--no more, no less. Otherwise they are "unaccountable" and are presumed to have obtained funding unfairly on the basis of false promises. As a result the perceived quality of paper plans tends to be more and more emphasized over the promise or track record of the investigator as a criterion for awards, because the evaluation of individuals is seen as more "subjective" than the evaluation of research plans, and hence potentially unfair.

Some devices, such as the MacArthur Prize Fellowships, the Waterman awards of the NSF, or the career investigator awards of both the voluntary health organizations and the NIH, are intended to counteract the bias towards favoring plans over past accomplishments, and this has been useful. Such mechanisms might be even more useful if quantitatively they constituted a larger fraction of the total and carried with them more realistic amounts of logistic support at the discretion of the investigator other than just the stipend of the individual. They could also be especially valuable if one of the criteria of selection included a definite preference for individual research styles which are selected against by the rest of the system.

More than twenty years ago, in a paper prepared for the House Science and Astronautics Committee in the volume Basic Research and National Goals, I wrote as follows: "I believe there is a most creative minority, possibly not more than 5% of all the active basic scientists, who should receive support for their work for no other reasons than their demonstrated capacity for original and creative work. This highly selected group of people might be provided with some minimal level of research support with no strings attached. They would simply be backed up to some level . . .to work on anything they thought worth doing. If they needed more than this, then their requirements would have to be justified in competition with others in terms of their specific proposed work and for other than purely cultural reasons." I suggested at the time (1965) that a federal agency could reasonably commit something of the order of" 5% of its academic research funding "to this type of completely freewheeling research expenditure." I see no reason to alter this recommendation today. I think it would go a long way towards alleviating the problem that Edelman identifies. Of course, such commitments should not be indefinite in time; they should be periodically re-evaluated, perhaps at intervals of no less than ten years. The interval should be long enough to allow for the gestation of really fundamental new ideas and insights, but should not be allowed to become a sinecure.

Having devoted most of my comments to agreeing with Dr. Edelman I should like to end by taking slight exception to a few of his statements about applied research and development. He makes two points that I feel are overgeneralizations, even though they are true in many specific cases. The first statement is that "technological pursuits yielding such profits are only rarely convertible into new scientific prophecies." The second is that "applied research and development . . . are carried out over short schedules of time at high pressure with well-understood scientific principles."

As to the first statement, in the field of CMP at least, the pursuit of technology has been just as important a new source of challenging basic scientific problems as basic investigations have been sources for new technology. The symbiosis of basic science and technology here has been remarkable. Many topics of basic research would never have been identified had they not showed up first as unexplained puzzles in technological development. The earliest efforts to develop a transistor, for example, opened up a whole new domain of surface physics whose existence was not even suspected before the invention of the transistor. As in many other cases this domain, once opened up, took on a life of its own and was pursued through many ramifications purely for its own sake in a depth that went way beyond that justified by the original applied motivation. Yet these ramifications kept periodically re-intersecting with applications and then diverging again over a period of thirty years.

A similar remark could be made about the phenomena of quantum-mechanical tunneling, first applied in the invention of the tunnel diode by Leo Esaki of IBM, but soon branching off as a self-propelling field of semiconductor physics which eventually moved off through a traceable genealogy into the discovery of the Josephson effect in junctions between superconductors and from there to the invention of Josephson junction devices and a whole new domain of electronic device and measurement technology.

Similarly, the demand for perfect crystals of germanium and silicon in the manufacture of semiconductor devices opened up a new field of fundamental investigation of imperfections in almost perfect crystals, which, once again, took on a life of its own as a new branch of science which only intermittently intersected with applications in many other fields, many having nothing to do with semiconductor devices or even semiconductors. Thus the fundamental investigations originating in the need to manufacture perfect crystals for semiconductor devices eventually found important applications in high temperature alloys for aircraft applications, in refined copper for vacuum switches, in the production of optical fibers, and in many other areas.

In addition, advances in science have become increasingly dependent on the penetration of new frontiers of measurement or data analysis capability. Although new instrumentation is usually developed first in the research laboratory, its commercialization by industry brings about a degree of reliability and simplicity in use which makes the techniques rapidly accessible to a much wider scientific community than would otherwise have been the case. Thus new techniques of experimental research rapidly become routine, enabling scientists to focus on fundamental conceptual issues without becoming unduly distracted by having to acquire extraordinary technical virtuosity in measurement itself.

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With respect to the second statement about the nature of development, while it may true of some macro-projects like Apollo or the Shuttle, the development process is frequently much more tortuous and serendipitous than Edelman's characterization would imply. Indeed, the more radical and far reaching an innovation, the more likely it is to be reached by a process much more akin to Edelman's description of basic science than to his description of applied research. Even developments that start out in a very orderly and planned way often have a much more chaotic and disorderly history than was anticipated. Well along towards commercialization the developers suddenly find themselves confronted with unexpected and critical gaps in the supposedly "well-understood scientific principles" that underlay their well-laid plans for technological development. The more radical, innovative, and far reaching the development, the more likely it is to have this kind of checkered history. Even in large scale military or space projects, the more the design presses on the existing state of the art, the more disorderly and basic-research-like development tends to become (and the more exasperating to management and to the politicians being asked to provide the resources). Albert Hirschman's famous "principle of the hiding hand" applies in spades to such developments, which require much more creativity, imagination, and ingenious improvisation than Edelman's description would seem to imply.

Thus I see a danger in Edelman's effort to draw too sharp a distinction between basic and applied research/development—a separation which may impoverish both activities in the long run. At the extremes of the basic/applied spectrum the differences in management styles and sociology are clear, but the gray area in between has been constantly broadening as technology becomes more science-based, and as scientific advance depends increasingly on advanced technology.

SCIENCE AND TECHNOLOGY

Britain's wasted research

Britain spends as much as any of its major trading competitors on research and development. It used to spend a good deal more (up to 2.7% of gross domestic product during the 1960s) than any comparable country. So why, if it spends so much on R and D, has British industry performed so poorly? After all, most studies suggest economic growth and R and D march hand in hand.

Addressing the Society for Chemical Industry in London last week, Sir Alan Cottrell, former chief scientific adviser to the British government, said it was not just that too much research has been siphoned off into non-commercial activities—defence, health, safety, environmental protection, law and order and higher education. Nor is it simply that precious resources have been squandered trying to outgun technological giants from the United States, in producing commercial white elephants like the VC-10 and Concorde airliners, the TSR2 bomber and advanced gas-cooled reactors for nuclear power stations.

Far more serious, in Sir Alan's view, is the way industry in Britain has been failing to generate enough fixed capital to commercialise the fruits of more than a fraction of the R and D carried out.

Around 28% of Britain's gross domestic product (£164 billion in 1979) is generated by manufacturing industry. The average life of its products varies—from five years for aerospace and electronic equipment, 10 years for cars and trucks to 15 years or more for machine tools and heavy machinery. Applying an average obsolescence rate of 10 years to Britain's £45 billion volume of manufactured output in 1979 indicates that £4.5 billion worth becomes unsaleable each year.

To produce new goods to replace obsolescent ones means extra investment. Sir Alan's rule is that roughly £1.5 of capital investment is needed to provide the equipment to produce £1 of extra annual net output. That suggests that nearly £7 billion of capital investment is required annually. On top of that, experience shows that about half as much again (£3 billion) is needed to provide all the necessary R and D (just over £1 billion) plus engineering design, testing, marketing, etc (nearly £2 billion). All told, then, Britain's manufacturing industry ought to be spending over £10 billion a year to keep its products up-to-date—ie, just to stay in its existing markets.

It is not. The rate at which Britain is investing at present means it can absorb only a minuscule amount of the R and D it carries out. Manufacturing industry's gross fixed capital formation was £6.6 billion in 1979, but replacing worn out equipment took £4.6 billion of that. Admittedly, some of this replacement would be made to coincide with innovation (eg, BL's £300m Metro car project) but how much is almost impossible to say. Leaving this aside, there is only £2 billion to carry out manufacturing industry's investment in facilities to make new products, compared with the £7 billion Sir Alan reckons the manufacturing sector needs to replace its obsolete wares.

One can begin to see why Britain's still relatively high expenditure on R and D may be largely wasted. On Sir Alan's figures, the £2 billion a year spent on net capital investment implies that industry is putting to work no more than £400m worth of the R and D that Britain does annually—ie, 0.24% of gdp. Yet British manufacturers devoted £2.3 billion to research and development in 1978.

On the other hand, the figures need to be studied sector by sector. Two thirds of manufacturing R and D is done by three industries (electronics, chemicals and aerospace), while 85% is done by just six industries.

Sir Alan spells it out

The sun

Taking its pulse

Astronomers think they have come up with a new way of probing the internal structure of the earth's own star, the sun. Telescopes reveal only the surface of the sun. But the nuclear furnace that powers the sun's output of heat and light lies at its core, buried below thousands of miles of dense, glowing gases. It is the physics of the core that scientists want to understand.

Traditionally, they have approached the problem by using mathematical models. Equations deduced from laboratory physics (describing rates of energy production and flow, pressure, density and other relevant parameters) are applied to a simplified model of a ball of hot gas. However, some of the assumptions used in the models are debatable. For example, nobody really knows the proportions of the different chemical elements present in the sun's interior.

In the 1960s, scientists hoped they could check their models by tracking the sun's output of neutrinos. Neutrinos are

DISCUSSION

Mr. **Mineta**. Dr. Brooks, thank you very much for your response to the questions that were proposed by the task force.

Let me ask—that it's been advocated that a fixed level of annual growth is the optimum way to establish the total level of funding for science, and you answered part of this. This approach is based on the thought that science itself expands at a fairly constant rate as new frontiers emerge and the frontiers opened in previous years continue to yield useful results. I wonder if you would care to comment on that assumption.

Dr. **Brooks**. I think the assumption and the argument are quite valid ones. However, there is one—I think I point out one problem with it, and that is that there are two things going on at the same time. On the one hand, previous scientific research is opening up new questions all the time, but at the same time certain fields do reach points of diminishing returns where, in fact, there are new questions opened up but they become more and more difficult and expensive to answer because most of the easy questions have already been answered. So there is a countervailing thing that there may be times when it is better not to keep answering new questions and try to answer new questions in a given field but to move on, move on to new fields, new issues where, in fact, the pickings are better. So that—but I think the answer to the question, to the specific question, is yes, an annual growth would be, I think, in principle a desirable policy. I am enough of a realist to believe that it is not probably a realistic goal.

And, furthermore, I think there probably should be fluctuations in the growth rate for research when very new, brand-new opportunities, these kinds like the discovery of a new load of ore, should be exploited when they appear. So I think any sort of formula like a fixed annual growth rate would in the long run really be a mistake.

Mr. **Mineta**. But I wonder if scientific expenditures are based on, let's say, the discovery of that new load of ore but rather on what is the growth of GNP or what is our sales curve growth. If our sales curve growth goes down, the first thing that gets cut back is R&D. Now we are in the era of Gramm-Rudman-Hollings, and so right now we are in a cutback, where we are in a cut-back kind of phase, and as I look at your statement on page 12 about public science being the kind of science that produces a public good, I think that the philosophical approach of this administration would be if it's worth doing, the private sector is going to do it, and so we ought to get out of the R&D field as much as possible.

Then when I look at page 2 of your statement, and you have it broken down between defense and nondefense in terms of basic, applied and development, I am just wondering whether or not we really aren't shortchanging ourselves in the long run?

Dr. **Brooks**. Well, I think here is the confusion between R&D and what I take the committee to mean by science and scientific research. I assume you don't mean the whole R&D budget by that but really what I would call the science budget, which is the sort of the $8 to $12 billion figure that I am talking about. And the characteristic of this part of the budget, the public good, that is sort of the public good part of the budget where, in fact, the applications run across a very wide spectrum of potential applications. Here, it is true that the private sector cannot appropriate the benefits because they spill over to competitors, to consumers, to other countries as we discovered in this country in the field of agricultural research more than 100 years ago.

I remind you that the history of the agricultural system in this country was that the wealthy farmers, mostly in the Midwest, tried to get together in the 1840's and 1850's and collect money from their members to spend on agricultural research. They were defeated by the free-rider problem, because everybody benefited from the money spent by those who were willing to contribute. They finally decided it was better to use the money to lobby the Federal Government to collect money from all the taxpayers to support research whose benefits were nonappropriable to any particular supporter of research. That is the system, it seems to me, with this $8 to $12 billion that I mention that is largely nonappropriable and, therefore, there has to be some way of collecting money from the free riders. So I think that is the place where I would agree with the administration's philosophy to some extent with respect to applied research and particularly to development of industrial applications, but I don't think it applies to the more basic end of the research spectrum or even to some parts of applied research.

Mr. **Mineta**. Given the growth in the federally funded science programs then we have seen, I think since the Second World War, this growth without any interruptions, do we have to continue to see that kind of growth in the funding of our scientific programs if we are to keep our technological leading edge role in science?

Dr. **Brooks**. Yes, I think so. Of course, the growth in real terms has not been all that great. In fact, there has been very little growth since about 1967 or 1968, and there was an actual shrinkage, particularly in the physical sciences, during the mid-1970's, so that the growth has been relatively small. Prior to 1967 it was quite large, and I think that some growth is necessary in the long run. But I would emphasize again my point that basic research, or research generally, may be a necessary but not a sufficient condition for maintaining our economic competitiveness, especially in the long run. Right now I believe our problem, our competitive problem is more related to what happens after research rather than to research. I think we shouldn't kid ourselves on that point.

Mr. **Mineta**. Isn't that maybe a short-run issue in terms of the research for competitive purposes versus basic reearch for the long run? And I am wondering whether or not—for instance, when our Science and Technology Committee was in Japan in January, one of the comments I made to them was the fact that I felt that the Japanese ought to be putting more money into basic research because we were doing the basic research, but they were appropriating for their own purposes our basic research and doing the developmental and applied research, that they would do more R&D on manufacturing and on process in terms of the amount of money they were putting into that part of it as compared to what we were doing.

Dr. **Brooks**. I would say where they are putting in money is even a little farther down the line. We are doing a lot of the development also, but they are doing the manufacturing and marketing, and I think they are converting the results of R&D into marketable products, good product designs and so on better than we are. But really that relates to what happens after, not just the basic research, but after the development part too.

Mr. MINETA. What about the fact there is more R&D money going into the defense sector than there is the nondefense sector? Does that give a tilt to what happens in the competitive marketplace?

Dr. BROOKS. Yes, I think it does. Of course, there are spillovers from defense, but you have to offset against that the opportunity costs of the very large number of scientists and engineers, particularly engineers, that are consumed and particularly consumed in the D part of the R&D. You have to remember that the defense, as my table in my statement showed—only 3 percent of defense R&D is basic research and all the rest is development, and a lot of it test and evaluation. That's an enormous consumer of manpower and resources. It tends to bid up salaries and make technical manpower more expensive, so our technical manpower is much more expensive relative to the rest of the population than is true in Japan. So there is a large opportunity cost, I think, associated with this defense R&D investment.

However, you have to put that in perspective. That's nowhere near as serious a problem today as it was, say, in the middle 1960's. We are still well below in percentage of our scientific resources in defense compared with what was the case with defense and space together in the middle 1960's when it reached a peak. But I think we are approaching a crisis again similar to what I believe we had in the middle 1960's, and I think some, at least, of our loss of competitiveness in the 1970's and 1980's was the result of the very large consumption of technical resources in the defense and space sector in the 1960's.

Mr. MINETA. While other industrial countries were trying to do it for the market side?

Dr. BROOKS. Yes.

Mr. MINETA. This would have been OK if you were in the market for a Stinger missle; but when is the last time you bought one at the department store?

What about duplication of scientific research? You touched on this a little bit. I am wondering, is competition between scientists stimulating and accelerating advances in science, or does that kind of duplication really cost us in terms of time and money, or do we have a proper balance in the scientific field given what you were talking about, the entrepreneurial spirit in R&D that you talked about?

Dr. BROOKS. Well, I think the problem with duplication is probably much more serious in development than it is in research because the more public—and what I call public knowledge by the very fact that it's public knowledge means that it is widely shared. And, as I pointed out, I think the peer review system, combined with the publication system and the refereeing of publications and so on, tends to create a strong incentive for scientists not to duplicate what other people are doing, unless there are good reasons to duplicate a critical experiment that needs to be repeated, and so on. So I think at the basic research end of the spectrum the problem of duplication—of course, there is some, but I don't think it's a major problem nor do I believe there are major resources to be saved by eliminating duplication in that part of the scientific enterprise.

Mr. **Mineta**. Dr. Brooks, you have been quoted as saying that most countries have better statistics on poultry production than on the activities of their scientists. I am wondering what specific kinds of statistics you would recommend to be added to the data series we now have about science and engineering?

Dr. **Brooks**. I guess I don't claim credit for that quote, but I think I have heard it. But I don't believe I am the source of it.

Mr. **Mineta**. Take credit. It sounds good.

Dr. **Brooks**. In one sense I think it's probably true, but science is much harder to measure than poultry, and I think we shouldn't pretend we can measure it like poultry. I think the *Science Indicator* series that is put out by NSF, which I had a hand in starting when I was a member of the National Science Board, is gradually improving its coverage. The difficulty with scientific statistics, however, is the fact that it's much easier to measure inputs than outputs; and, furthermore, you are measuring an activity that is different all the time. I mean if you are measuring—you know, the chickens change over time but they don't change that much, but the output of research changes over time. A scientific result that would be regarded as totally trivial today would have been a major job to have done, to have gotten that kind of result 15 or 20 years ago. In other words, you are never measuring the same activity. When you add computers to instrumentation and so on, your ability to get data, to analyze data, to sort data, to pick out unusual events and so on has increased enormously, so it it's very, very difficult to know exactly what it is you are measuring, even when you are measuring inputs.

I don't really have any suggestions regarding new kinds of data to collect. The one thing I have advocated that I think would be worth doing more of is to look retrospectively at the evaluation of such projects after the fact. We fund research projects on the basis of what they promise to accomplish with a careful review. We much less frequently go back and look 10 or 15 years later at some of those projects and see if they produced. Were they as promising, or were they as productive, as the peer reviewers said when they evaluated the projects? I think we need some work in what I would say is validating the peer review process retrospectively.

Mr. **Mineta**. You mean there are more at the qualitative part rather than the quantitative part of it?

Dr. **Brooks**. Yes.

Mr. **Mineta**. What kinds of measuring tools do you have other than, let's say, did this research accomplish what it intended? Other than that kind of peer review or measuring it against what was originally—any other ways to measure that quantitatively?

Dr. **Brooks**. As far as whether it accomplished what it intended—in the light of what we know now, was it as important as we thought it was then? Was the accomplishment that was intended that important to science in retrospect?

Mr. **Mineta**. Dr. Brooks, thank you very, very much.

I am going to now turn it over to our very distinguished and fine chairman. I am going to have to run off. I appreciate your being here. Let me introduce you to Dr. Fuqua.

Mr. **Fuqua** [presiding]. Thank you, Dr. Brooks. I don't have any questions. I apologize for being late. I was testifying over in the Senate this morning. Thank you very much for showing up.

Mr. **Fuqua**. Our next witness would be Dr. Martin Baily, a senior fellow at the Brookings Institution. We are pleased to hear from you this morning.

[A biographical sketch of Dr. Baily follows:]

Dr. Martin Neil Baily

Dr. Martin Neil Baily was educated at Cambridge University and MIT. He completed his undergraduate education in 1967, receiving a college prize in economics, and his Ph.D. in 1972. He taught at MIT and Yale before becoming a Senior Fellow at the Brookings Institution in 1979.

His areas of expertise include innovation and productivity growth, monetary and fiscal policy, and the labor market. He has testified to Congress on economic policy and served as an outside adviser to the Federal Reserve Board and the Congressional Budget Office He has been a consultant to several major corporations and is currently advising a consortium of high-tech companies. He has been a member of the Brookings Panel on Economic Activity since 1976 and of the Economic Fluctuations Program of the National Bureau of Economic Research since 1978. The results of his research have been reported in the *New York Times, Business Week* and many other places. He has published two books and numerous professional papers.

Since 1979 his major concern has been an investigation of the slowdown in productivity growth in the U.S. He is currently studying innovation in U.S. industry and investigating its relationship to productivity. He has been involved in a major data collection effort to determine directly the pace and nature of innovation in recent years. Evidence was presented to the Brookings Panel in the fall of 1985 indicating that there was a decline in innovation in the 1970's that has also contributed to the slowdown in productivity growth.

STATEMENT OF DR. MARTIN NEIL BAILY, SENIOR FELLOW, BROOKINGS INSTITUTION, WASHINGTON, DC

Dr. **Baily**. Thank you.

I am an economist and I am going to talk about sort of the receiving end, the consumers of the technology, if you like, in terms of economic performance, economic growth, productivity, a little bit about competitiveness. I have a written statement. I will try and draw out a few of the highlights from it. I certainly don't think it's worth sitting here and reading it.

Mr. **Fuqua**. We will make the entire statement a part of the record. If you would summarize, that would be fine.

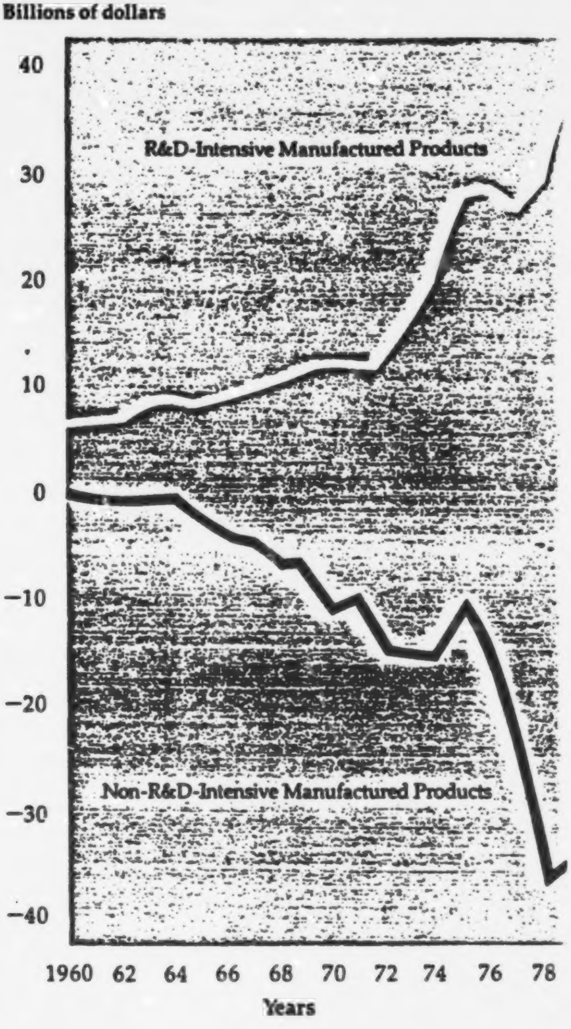
Dr. **Baily**. Thank you.

The first point I make is the U.S. economy does appear to be stuck on a path of very low productivity growth, and we keep thinking that is going to end—we are going to turn around—and it doesn't seem to be doing it. Just to give an idea of the order of magnitude, if productivity growth had continued at the same pace after 1973 as it had achieved over the period 1957 to 1973, our output would have been about 25 percent higher last year than it was with the same level of employment. That's a huge loss of output if you want to think of it that way. It would have been enough to solve the budget crisis several times over. We would have more consumption, more investment.

We didn't get that extra growth. I am just trying to point out by the magnitude that it is a very important problem, and anything that can be done to raise the rate of productivity growth will be an important contribution to the economy and to solving a lot of diverse problems.

Innovation has perhaps the key role to play in both productivity growth and competitiveness. New technology in the form of new products, and new processes, is the catalyst of, the origin of, most of the productivity advance that we achieved. Innovation is just as important to foreign trade, and I have in the testimony a figure that comes from the National Science Foundation showing how the United States became a very much net exporter of high technology goods in the 1970's and a net importer of low technology goods.

Figure 1.
U.S. Trade Balance in R&D-Intensive and Non-R&D-Intensive Manufactured Product Groups

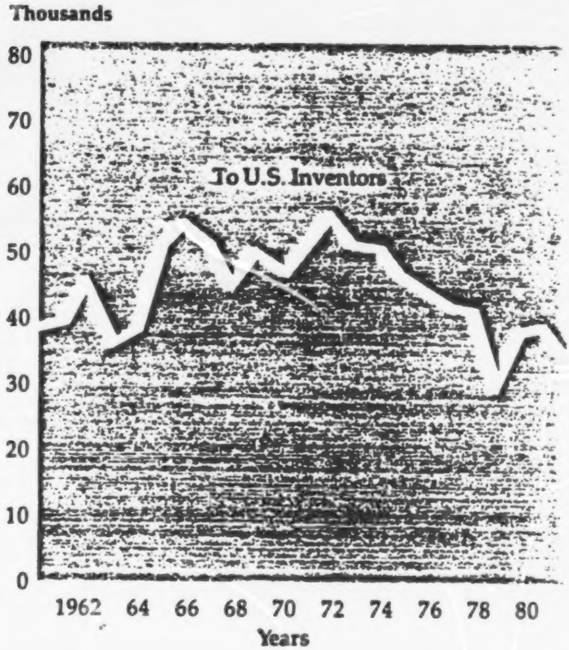


Source: National Science Board, *Science Indicators 1980*, Government Printing Office, 1981, p. 32 (Figure 1-12).

There is a more recent figure which I did not include for the last few years which shows beginning—that doesn't look quite the same way. The reason I didn't include it is because the last few years have been so affected by the overvalued dollar it's almost like holding a magnet next to the television set. The whole picture has been distorted downward, but it still remains true that the United States has a comparative advantage in high technology items, so that maintaining a technological lead in many areas or enough areas is really a key to our competitiveness as an economy.

Now, one of the reasons I think for the slowing of productivity growth, and one of the reasons for the increasing difficulties we have had in international competitiveness, aside from the overvalued dollar, is that the pace of innovation has slowed down. I point to a couple hints—and I use the word hint advisedly. I really don't think we have solid measures of the output of innovation. I point to a couple of hints this has happened. One is the flow of patents and that peaked in the early 1970's and has been declining since then.

Figure 2.
Patents Granted to U.S. Inventors, 1960–82



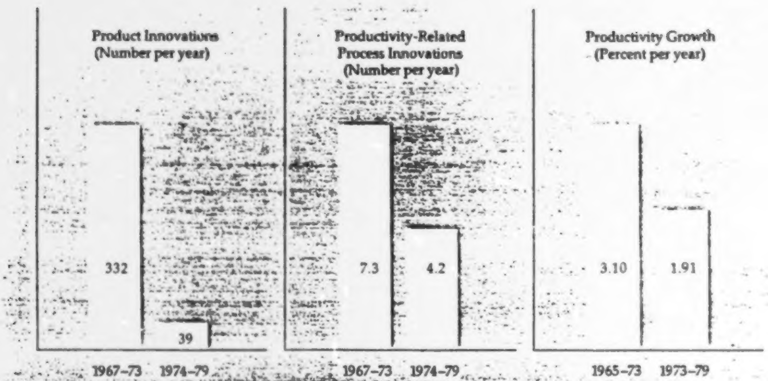
Source: National Science Board, *Science Indicators 1982*, Government Printing Office, 1983, p. 101 (Figure 4-8).

One of my colleagues at Brookings, Mike Scherer, has looked at other countries too and found that we are not alone in that, we are not the only country where the rate of patenting has peaked. Most of the European countries have had a decline in their own rates of patenting, some of them even more dramatic than ours. The only country, interestingly enough, that hasn't had such a decline in patenting is Japan. There is some doubt about the comparability of Japan's patent statistics since the Japanese tend to patent everything where other people only patent more significant items. But, nevertheless, the general picture is one of a decline in the rate of patenting by U.S. inventors that is paralleled in Europe, and perhaps in Japan less so.

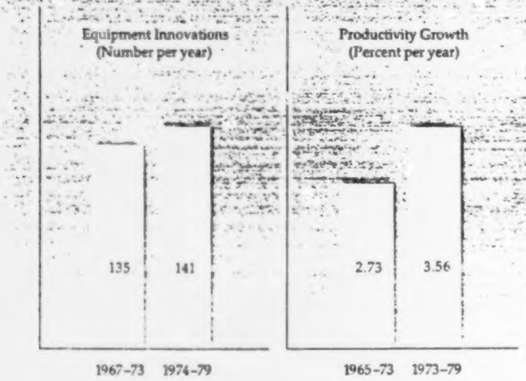
The second hint comes from some work that I did with a coauthor in which we looked at innovation and we tried to get direct measures of innovation for a couple of industries, and two swallows don't make a summer, but I think these two industries were at least indicative of what might be happening elsewhere in manufacturing.

We found a very marked relationship between the pattern of innovation and the pattern of productivity growth. The textile industry had a continued flow of innovation—not, I may say, generated within that industry but generated by machinery suppliers outside the industry, the textile industry had adopted a continuing flow of innovations and had maintained rapid productivity. The chemical industry, which does most of its own internal development of technology, had suffered low productivity and the decline in the pace of its innovation.

Figure 3.
Innovation and productivity both slowed in the chemical industry…



but not in the textile industry



Source: Martin Neil Baily and Alok K. Chakrabarti, "Innovation and Productivity in U.S. Industry," *Brookings Papers on Economic Activity*, 2: 1985 (forthcoming).

Since the majority of the industries in U.S. manufacturing did suffer a slowdown in productivity growth in the 1970's, I think the inference from this—and it's a tentative inference because of the very, very small sample, but the inference is a decline in the pace of innovation was a factor in the decline in productivity growth in manufacturing, and potentially in the rest of the economy, because the new products—the new technologies—developed in manufacturing are important outside that sector.

Why is it that innovation has slowed down? Part of the answer to that has come from some interviewing and case studies that we did of a number of U.S. companies. It was attributed particularly in the chemical industry, which was the one that had demonstrated the slowdown in innovation. There was a general sentiment, which we heard from a number of people in R&D, a number of people in the production end, and from virtually all the companies that we spoke with, that there had been a maturing of the technology in a number of areas. At the end of World War II, there were just tremendous opportunities for development. In the 1930's, much development had been inhibited by the Depression, by lack of funds. World War II had created this kind of forced draft of scientific development, but geared to defense production rather than domestic production. So then at the end of the World War II, there was just almost a smorgasbord of opportunities where innovation, where R&D, could go in, and a lot of new ideas, a major breakthrough in technology could take place. That period really lasted two decades or more, and fueled innovation and productivity growth really through the 1950's and 1960's into the beginning of the 1970's.

Now, that is one answer, but as several people said to me in these interviews, and as, I think, common sense suggests, that if you have a maturing of technology in some areas, that doesn't necessarily mean there are no opportunities for advance. It may suggest that you should look elsewhere to get your new scientific advances. We do have really a premier scientific community in the world which has generated a tremendous flow of new science, and, I think, part of the problem was that we were not able to take this flow of new basic science and transform it as rapidly as we might have into new technological developments. There is quite a big step to go from basic science how this is going to be applied, and what general ways can this be applied technology, and then when the companies do have much applied R&D, how you turn it into commercial products which can be marketed out to consumers and to other companies.

One of the reasons we failed in this, I think, is because company-financed R&D didn't grow as rapidly after 1967 as it had grown before that. It did grow, but there seems to be—and I think there was some reference to this earlier—there seems to be a sort of natural law that you have. It's like Alice in Wonderland. You have to keep running faster and faster to stand still. You have to keep company-funded R&D growing more rapidly than GNP if you are going to maintain the same pace of innovation in the economy, and we failed to do that. Company-funded R&D grew no faster than GNP over this period. So that was one of the reasons, I think, we failed to make that transfer over into new technologies.

The other reason, I think, is because there was not enough resources devoted to the sort of middle step where you have, not pure research, but sort of generic applied research, the kind of thing the companies have trouble doing on their own because they can't appropriate the benefits. But it's not necessarily defense—related; it may be more civilian related, and I don't think there were enough resources put into that.

All right. Let me say now some suggestions, taking science policy broadly, not just the funding of the pure research, but why, I think, science and technology policy could help in the future in terms of stimulating greater productivity growth.

First of all, there is only a certain amount, I think, science policy can do. It's a huge private economy out there, the world economy; it's not going to be something where a small change in policy is going to make a huge difference. But, I think, we should be pushing in the right direction. We can make a significant contribution, and I have three suggestions really.

The first relates to what I just said, and that is we need to step up our funding of applied technology development. Recent history demonstrates the Government has played a vital role in nurturing high technology. It's so important because there is a barrier which private companies can't overcome to the opening up of major new areas of opportunity. You are going to need massive resources, the risks are extremely high, and once the progress has been made, any group that pioneered in development is probably not going to be able to appropriate its benefits.

I think there is recognition here in Washington of the need to do this. There is some in the direction in which defense R&D is pointed. Many people cite the Strategic Defense Initiative as being partly a program for the development of the computer industry as much as it is a development of a weapons system. The National Science Foundation believes that it's got to fund its own activities more in the direction of generic technology development, try to promote new ways of approaching engineering and scientific problems; but, I think, funding does need to rise in this area.

I think in order to do that, we do have to have some evaluation of how the Government's role should be structured, so I am going to sound a warning here. The history of Government efforts to push forward technology is one of great successes, but also staggering failures. We developed the atomic bomb, and the computer, and we sent a man to the Moon, but the breeder reactor absorbed billions of dollars before expiring, and the synfuels project has done no better. Even before the recent disaster, the Space Shuttle had been much more expensive and less reliable than hoped.

It is in the nature of this task you are going to get spectacular successes and failures. After all, the whole reason you have got a Government role in this area is because of the high risk and the large-scale investments. But what we need to look at is what institutional framework we can use that will minimize the disastrous losses. The problem is that powerful groups and individuals develop vested interests in continuing projects that are going nowhere. To point to one in particular, I think the breeder reactor program lasted more because of its ability to generate jobs in Tennessee than because of its technological potential.

The second suggestion I make for how science policy could help innovation, and help economic growth, is by helping us to use the research done overseas. Now we have had some reference to this before. It is the case that other countries, both in Europe and Japan and even Eastern Europe and the Soviet Union, have really been stepping up their own scientific and R&D resources. There are about as many scientists and engineers engaged in R&D in France, West Germany, Japan, and the United Kingdom, as there are in the United States. So there is a research community out there which is at least as large as ours, and we are not so chauvinistic as to think ourselves so much better we don't need to look at it. We have certainly learned better than that.

I found, in talking to businesses, a surprising reluctance to acknowledge the importance of foreign technology. One company that I spoke with was almost apologetic about the fact they were monitoring technological developments in Japan. That seemed to me to be absurd. Japanese companies have been monitoring our technological developments for years and making money off them, and for us to get a reverse flow seems to me to be only sensible.

One of the issues in research in R&D is that the benefits spill over. Well, if the benefits from our research spill over to other countries, of course it can be true that the benefits of their research can spill over to us. We can find out what is being done there to some extent, at least keep up with what is available.

So I suggest information centers. I don't know what is a good name for it, but these information centers would be established to really reduce the costs to U.S. scientists and engineers of finding out about foreign scientific and technical developments. The language barrier itself is one issue, and Senator Rockefeller has introduced legislation to ask for the translation of Japanese journals. That is a good step. There is nothing wrong with that. My only concern is that, judging from my own experience, my in-tray gets piled up with journal after journal that I am supposed to read, and each one takes a long time, I think, as much as anything, what is needed is the ability to digest this information. If we are going to tell our scientists and engineers: "You have to keep abreast of what is going on in the whole world," I don't think they would have time to do their own research. I think you have to take this information, not only translate, but help digest it, to process, to filter out what is important from what is trivial—and there is a lot of trivial research done everywhere, here and overseas—and then make available as far as you are able to do so a flow of information: Briefings, summaries, seminars, conferences to keep our own scientists and engineers abreast of what is going on. Of course, some of that already goes on. I am not saying that is a brandnew thing. I think it is something that should be encouraged and supported.

The final element of the science policy is a little bit separated perhaps from the work of this task force, but I think it's important and I want to mention it. It is that I do think some tax incentives for private R&D are necessary. Now, there is a spectrum here. We have basic research, we have sort of applied technology development of a sort of general nature, and then we have company R&D which is mostly to do with the development of new products and processes, very much market oriented, economic-bottom-line oriented. I think there is a clear role for direct Government funding of the first two elements—the middle one in combination with the private sector—some joint ventures involving some Government funding, some cooperative private funding for these technology developments; but when it comes to the company funded R&D, I still think there is a small role for a Government tax incentive. It's not a place for direct funding, but I think it is a place for a tax incentive.

Why is that? It's again because of the spillovers. Even though company-funded R&D is commercially oriented, is market oriented, it is still the case that the results of that R&D will be copied by other companies, including other foreign companies. It's still the case that key personnel within your own company may work on your research program, and then the next thing you know they have moved to another company or set up their own company, so the fruits of what you have been funding then go outside your own company.

The University of Pennsylvania economist, Edwin Mansfield, has estimated the social return, return to all of us, from company-funded R&D is about twice the private return, and that's a market failure in the sense there is inadequate incentive provided to the private sector for doing R&D. So I support the existence of an R&D tax credit, or some similar device, to just increase the amount, if we can, of company-funded R&D.

Thank you. I'll stop there.

[The prepared statement of Dr. Baily follows:]

Statement to the Task Force on Science Policy
# of the Committee on Science and Technology
U.S. House of Representatives

by

Martin Neil Baily*
The Brookings Institution

Tuesday, April 15, 1986

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*This statement draws on my article "Innovation and U.S. Competitiveness," Brookings Review, Fall 1985, co-authored with Alok K. Chakrabarti. The views expressed are my own and should not be ascribed to the trustees or other staff members of the Brookings Institution.

The U.S. economy remains stuck on a path of very low productivity growth. From 1973 to 1985, output per hour in the non-farm business sector grew at only 0.7 percent per year compared with 2.5 percent a year over the period 1957-73. The cumulative impact of this slowdown has been tremendous. Had growth continued at the same pace after 1973, output would have been almost 25 percent higher in 1985 than it actually was, with the same level of employment. This additional output could have solved the budget crisis several times over and still left everyone better off.

The Role of Innovation

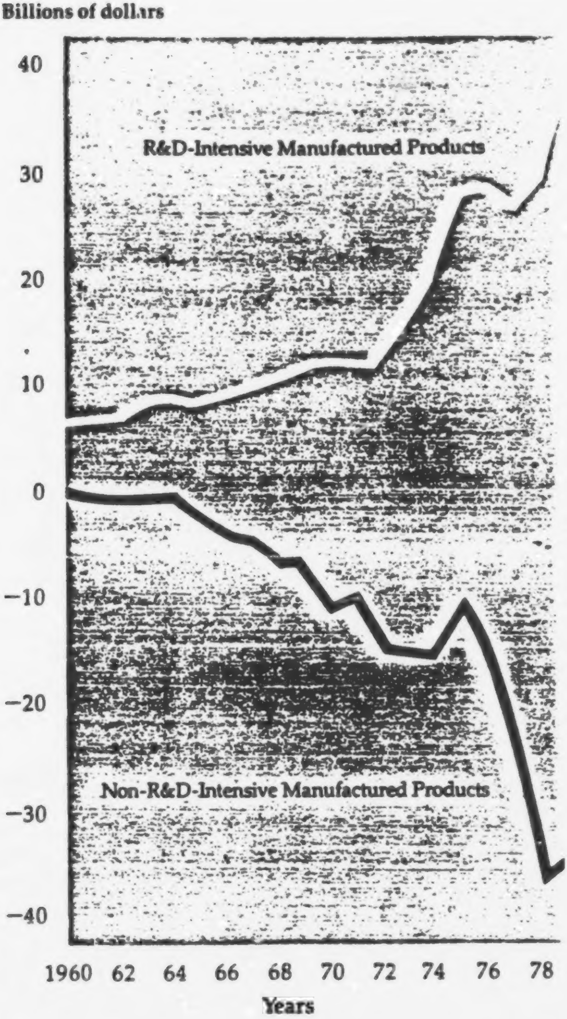
The analyses conducted in the 1950s of economic growth were built on the idea that saving and capital investment are the engines of growth. Moreover, economic policymaking in the postwar era has been predicated on the assumption that productivity is enhanced primarily by new plants and equipment. Evidence has accumulated, however, that new technology may be an even more important source of growth. New products and new methods of production are the origin of or the catalyst for productivity advance. For example, Brookings economist Edward Denison finds that only 20 percent of the increase in output per employee over the period 1929-82 was the result purely of increased capital per worker.[1] Innovation contributes to growth as new ideas are applied directly to improve productive efficiency and as new technology is incorporated into new equipment and new plants. Indeed, without the flow of new technology, capital spending itself tends to dry up. The expansion of capital, as an end in itself, is only a minor motive for investment; the main motive is that new equipment and new technology are purchased together.

Innovation is just as important to U.S. foreign trade. The National Science Foundation has charted with startling clarity (see Figure 1) the extent to which the United States became a net exporter of high-technology products and a net importer of other products in the 1970s. This theme has been reinforced by Brookings author Robert Lawrence, who has also found that high-tech industries have weathered the effects of the overvaluation of the dollar in the 1980s better than other industries.[2]

The Slowing of Innovation

Since innovation is a key source of productivity growth, it is natural to expect that some part of the growth slowdown is associated with a reduction in the pace of innovation. And there are two important hints that this is so. The first of these has also been charted by the National Science Foundation (see Figure 2). Patents granted to U.S. inventors peaked in 1972 and declined substantially over the next ten years.

Figure 1.
U.S. Trade Balance in R&D-Intensive and Non-R&D-Intensive Manufactured Product Groups



Source: National Science Board, *Science Indicators 1980,* Government Printing Office, 1981, p. 32 (Figure 1-12).

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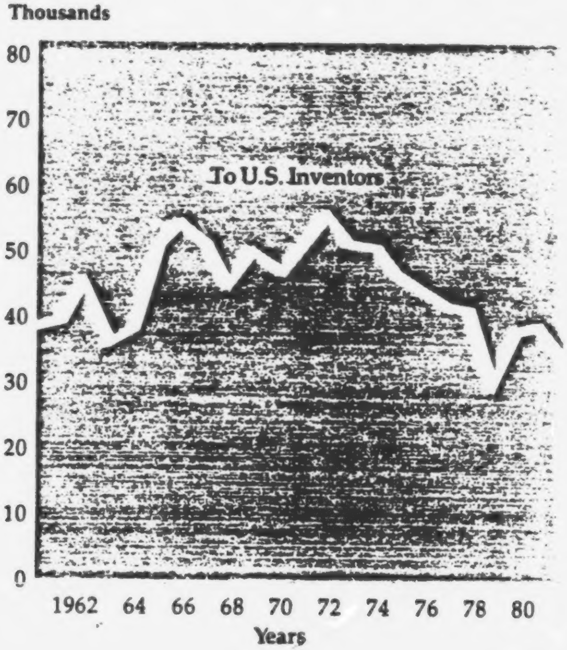
Figure 2.
Patents Granted to U.S. Inventors, 1960–82



Source: National Science Board, *Science Indicators 1982*, Government Printing Office, 198[illegible] p. 101 (Figure 4-8).

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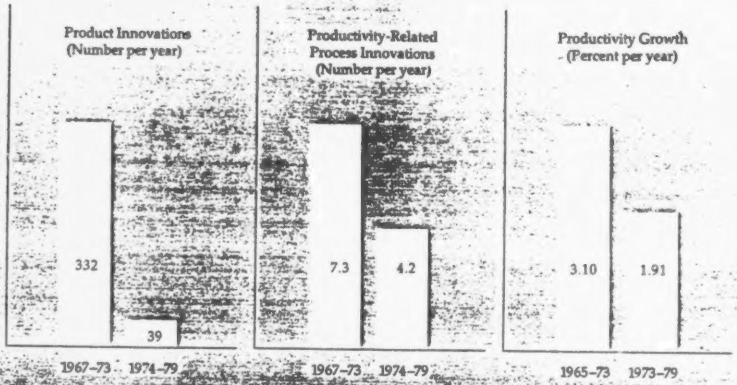
The second hint comes from work I have done on selected manufacturing industries. In collaboration with a co-author, I analyzed two manufacturing industries: the chemical industry, which had a substantial productivity slowdown in the 1970s, and the textile industry, which did not. We collected information on the flow of innovation in each industry and found a marked relation between innovation and productivity trends. The industry in which productivity slowed -- chemicals -- was also the industry in which innovation slowed. Figure 3 illustrates this.

Since the majority of industries in U.S. manufacturing (and many others, too) had productivity experiences more like that in chemicals than that in textiles, these findings support the view that innovation slowed in the 1970s and pulled productivity growth down with it.

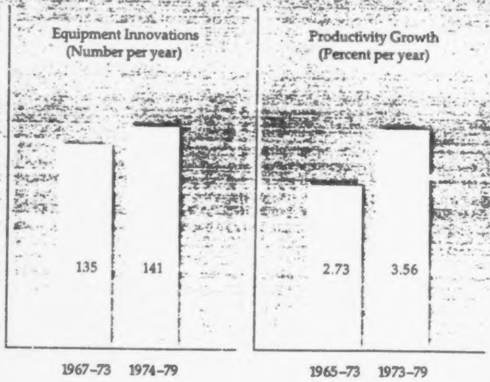
Reasons for Weak Innovation

The slackening of innovation is due in part to the maturing of technology in many fields. The Great Depression of the 1930s disrupted the economy, deprived the business community of resources necessary for development, and discouraged risk-taking. World War II created a forced draft of scientific and technological development, but with applications geared to war production. As a result, the postwar period was marked by tremendous opportunities for the commercial development

Figure [illegible].
Innovation and productivity both slowed in the chemical industry . . .



. . . but not in the textile industry



Source: Martin Neil Baily and Alok K. Chakrabarti, "Innovation and Productivity in U.S. Industry," *Brookings Papers on Economic Activity*, 2: 1985 (forthcoming).

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of technology, and these opportunities fueled innovation and productivity growth for two decades.

However, the natural maturing of technology is not an entirely satisfactory explanation for weak innovation. Indeed, the maturing of one direction for progress could have served as a challenge to firms to chart other directions. But in fact this did not happen often enough. Companies decided that the return to technological development had fallen, and they reduced their commitment to it. Rising inflation, together with tax provisions allowing all interest payments to be deducted from income, made it more attractive to purchase safe assets that could be highly leveraged -- and decreased the appeal of risky investment in new technology. One sign of this is that company-financed R&D spending grew no faster than GNP over the period 1967-77.[3] There seems to be a natural law that R&D spending has to grow faster than output if the pace of innovation is to be kept up.[4] When this acceleration did not materialize in the early 1970s, the pace of innovation fell.

In fairness to the business community, it should be said that external factors played an important role in this decline. Companies had to deal with inflation, deep recession, sharply increased energy costs, and proliferating environmental and worker-safety regulations. The squeeze on R&D budgets came from a squeeze on profits. And technical and managerial resources were diverted away from their normal tasks to deal with energy or environmental problems. Beginning in 1977-78, R&D spending increased rapidly again as companies realized the importance of new technology to growth and profitability.

Nearly all R&D spending takes place in manufacturing, and there is some sign that the turnaround in corporate R&D outlays is showing up in better productivity growth in manufacturing since 1979. Unfortunately, however, growth in the rest of the non-farm business sector remains extraordinarily weak. There is no sign yet that an improvement in the pace of innovation within manufacturing is helping productivity outside this industry.

Science Policy and Economic Growth

The Agenda for a Study of Government Science Policy prepared for this task force makes it clear that government supports science largely because of its beneficial effects on the economy. Certainly there is a role for government support of pure research, just as there is a role for government support of symphony orchestras. But with this motivation alone, the level of support would be low. Scientific endeavor is expected to earn its keep, by providing the advances in knowledge that will eventually result in innovation, and by training the scientists and engineers that work in industry.

And there is some direct support for this idea. A group of economists at Yale have surveyed firms in several industries and found that they depend importantly on the flow of new science and newly trained scientists and engineers that are generated within our universities and research labs.

Two questions, therefore, come to mind with respect to science policy. Did bad science policies contribute to the innovation and productivity slowdowns in an important way? And could positive policies help improve the outlook for growth in the future? The answers to these questions are no and yes.

If one wishes to blame bad policy for the adverse economic performance of the past 15 years, one could point to the government's partial responsibility for inflation, recession and energy prices. Thee was no dramatic shift in science policy in the 1970s that could explain a decline in innovation.

Science policy can help reverse the slowdown by expanding the opportunities for technological advance and economizing the private sector to exploit the opportunities that exist. There are three potentially fruitful approaches to doing this. First, there should be direct funding of major applied technology developments in areas where no private company can succeed on its own. Second, Information Centers should be established to allow U.S. scientists and engineers better access to world-wide developments. Third, tax incentives should be provided for private sector R&D.

Direct Funding of Applied Technology Development

Direct Federal support for research and development has grown rapidly since 1980 in the ares of defense[5] and basic non-defense research.[6] However, funds for applied research and development in the non-defense area have dropped sharply 1980-85, after adjusting for inflation. This is a move in the wrong direction if the goal is to reverse the decline in innovation opportunities available to industry. Recent history demonstrates that the government has played a vital role in nurturing high technology in the United States.[7] Programs in the Defense Department were instrumental in pushing forward the development of early computers, and the aircraft industry continues to be aided by defense money for development. Government's role is so important because there is often a barrier, which private companies cannot overcome, to opening up major new areas of opportunity. Massive resources may be necessary, the risks are extremely high, and once progress has been made, the group that pioneered a development may well not be able to appropriate its benefits.

There is some recognition in Washington of the need to provide an impetus for technological development. The National Science Foundation has shifted its orientation and is increasingly concerned with how scientific development can help the economy. It is setting up centers for "generic" technology development, that try to promote new ways of approaching engineering and scientific problems.

Funding needs to rise in this area, not fall, however, and if this is to happen, an evaluation is needed of how the government's role should be structured. The history of government efforts to push forward technology is one of great successes and staggering failures. The atom bomb and the computer were developed; a man was sent to the moon. But the breeder reactor absorbed billions before expiring, and the synfuels project has done no better. Even before the recent disaster, the space shuttle had been much more expensive and less reliable than hoped.

It is in the nature of the task that spectacular successes and failures will occur. After all, government has a role precisely because of the high risks entailed and the large-scale investments required. But, clearly, an institutional framework that can minimize disastrous losses has not been established. Powerful groups and individuals develop vested interests in continuing projects that are going nowhere. The breeder reactor program lasted more because of its ability to generate jobs in Tennessee than because of its technological potential.

Economists have long been preoccupied with policies that control the business cycle or improve static efficiency, but only a few focus on policies for growth.[8] The institutional structure of the Federal

Reserve Board has been the subject of exhaustive discourse. But the mechanisms for stimulating technological development have remained on the periphery of economic debate. That must be changed.

Using the Research Done Overseas

Information Centers should be established in key scientific and technological areas that would gather, digest and disseminate information from around the world. The United States is no longer alone on the frontier in many areas of science and technology. Europe and Japan have developed their own scientific bases, and industrial R&D has grown rapidly in these countries also. In 1981 there were 627,000 scientists and engineers engaged in R&D in France, West Germany, Japan, and the United Kingdom, compared with 691,000 in the United States. The catching-up of other countries is a threat to the competitive position of our economy, but it is also a potential benefit to us. The United States no longer has to do all the work. We can learn from others.

The Information Centers would lower the costs to U.S. scientists and engineers of finding out about foreign scientific and technical advances. The language barrier alone provides a formidable obstacle to the spread of knowledge. This is particularly true for Japanese developments. In part, we are a victim of our own success. Foreigners learn English to keep abreast of U.S. developments, but we do not learn other languages to the same extent. Making translations of books and articles available is one way to solve this problem. But more is needed. It is too hard to keep abreast of the literature on a worldwide basis. The Centers should also contribute to the task of digesting the flow of information by providing briefings about and summaries of foreign developments.

The Centers need not be a drain on taxpayers. They could offer their services for a fee to industrial and university subscribers.

Tax Incentives for R&D[9]

From 1981 to 1985 the tax code included a tax credit for R&D. This credit gave about a 7 percent incentive to companies to perform R&D, meaning that the rate of return to projects was increased by the credit by about 7 percent. It is hard to unscramble all of the influences on R&D spending, but the weight of the evidence is that the credit led to an increase in spending by U.S. companies. There is a strong economic case for providing such an incentive to private R&D.

University of Pennsylvania economist Edwin Mansfield has found that the return to society from industrial innovation is much higher than the return to the innovating company. Competitors quickly copy new products, and key personnel change companies or set up their own companies, taking their knowledge with them. The rapid flow of technological information overseas means that repetitions of new products often come from overseas and further erode the returns to U.S. innovators. This diffusion of knowledge benefits consumers because the competition that it creates helps to bring prices down. But the consequently short half-life of a technological edge in the modern world leads to underinvestment in R&D. The private market fails to provide an adequate incentive for innovation.

In these circumstances, it is appropriate for tax policy to give to the private sector an additional stimulus over and above the market incentive. Some economists have expressed concern about the effectiveness of the R&D tax credit and about the possibility that companies abuse it by classifying marketing expenditures as R&D. Indeed, based on these concerns, proposals have been made to reduce or eliminate the current incentive provisions. I believe that provided the tax incentives can be properly targeted, they should be retained or even strengthened. Efforts to reduce the budget deficit, which I strongly support, should not eliminate the modest pro-growth provisions now in the tax code.

Conclusions

Science policy is a matter of vital interest because of the importance of scientific and technological advance to economic growth and prosperity. The past 15 years have been ones of poor economic performance, with slow productivity growth caused in part by weak innovation.

Science policy was probably not a major cause of the decline in performance. But in the belief that important scientific breakthroughs and major innovations remain to be discovered and developed, science policy can play a beneficial role. Three policy initiatives have been suggested here. First, direct sponsorship of applied research and development should be increased. Second, Information Centers should be established as a way of benefitting from the vast resources now being devoted to science and R&D in the rest of the world. Third, tax incentives for commercial R&D should be restored.

At a a time of huge budget deficits, a plea for additional funding for science and technology may fall on deaf ears. I hope not, because stimulating an increase in economic growth is the best way of solving the deficit problem in the long run.

NOTES

1. Edward R. Denison, Trends in American Economic Growth, 1929-1982, Brookings, 1985, table 8-3, p.113.

2. Robert Z. Lawrence, Can America Compete?, Brookings, 1984. Recent U.S. trade patterns are described in U.S. Department of Commerce, United States Trade: Performance in 1984 and Outlook, Washington, D.C., June, 1985.

3. National Science Board, Science Indicators 1982, Government Printing Office, 1983, table 2-2, p.234.

4. This law has been propounded by F.M. Scherer, now a visiting scholar at Brookings.

5. Money for defense R&D goes almost entirely into development.

6. See National Science Board, Science Indicators: The 1985 Report, Government Printing Office, 1986, figure 2-4, p.39.

7. See Jerome E. Schnee, "Government Programs and the Growth of High Technology Industries," Research Policy, Vol. 7, 1978, pp. 2-24, and Alok K. Chakrabarti and William E. Souder, "Critical Factors in Technological Innovation and Their Policy Implications," Technovation, Vol. 2, 1984, pp.255-275.

8. One economist who does look at these issues is Richard R. Nelson. See, for example, High Technology Policies; A Five-Nation Comparison, American Enterprise Institute, 1984.

9. In collaboration with Robert Lawrence of Brookings, I have prepared a study on the R&D tax credit that was sponsored by the Coalition for the Advancement of Industrial Technology. This testimony is in no way sponsored or endorsed by this group, however.

Innovation and U.S. Competitiveness

Martin Neil Baily
and
Alok K. Chakrabarti

THERE IS no question that the immediate crisis of U.S. trade is primarily a result of the overvalued dollar. And there is no question that reducing the budget deficit is an essential prerequisite for solving this crisis. But the severity of the current problem should not blind us to the longer-term issue of the competitiveness of U.S. industry.

The United States has traditionally been able to pay higher wages than other countries and yet still compete internationally because of superior technology. Productivity was higher than in other countries, and often the items produced were more advanced and could command premium prices. Because of its technological sophistication, the United States was able to develop a unique position in the world economy, and U.S. industries and workers were the beneficiaries.

The unique status of the United States has been eroding, as other countries have been catching up in productivity and product technology. From 1973 to 1984, output per hour in manufacturing grew at only 2.0 percent a year in the United States, compared to 6.7 percent in Japan, 4.5 percent in France, and 3.5 percent in Germany. R&D spending in these countries has also risen faster than our own, and important innovations are being made first overseas. The fraction of U.S. patents granted to foreigners rose from 16 percent in 1960 to 41 percent in 1982.[1]

When productivity is rising faster in other major industrial countries than it is in the United States, then U.S. wages and salaries have to decline relative to those elsewhere, after adjusting for inflation, in order for the United States to remain competitive. And when important U.S. industries lack a decisive technological advantage over competitors in low-wage developing countries, then imports come flooding in, causing U.S. job losses and generating political pressures for restrictive tariffs or quotas.

U.S. industry thus has serious problems, but it also has important strengths. Although it is not growing rapidly, the *level* of productivity in the United States is as high or higher than that achieved overseas in most industries. The United States has an unparalleled educational and scientific base, and it puts about as many resources into R&D as Japan, Germany, and France combined.[2] Moreover, America has developed its high-technology industries rapidly, whereas Europe has not. Seventy percent of all European computer equipment is U.S.-built or supplied by American companies.[3] The much-feared Japanese effort to build a super computer, aided by a government grant of $500 million over 10 years, has to be weighed against the $3 billion that IBM spent on R&D in 1984 alone.[4]

The overall picture, therefore, is one of both significant problems and substantial strengths. We will explore here some of the facts behind the problems and some public and private policies that might be used to help build on the strengths. The emphasis of our own work has been on innovation. We believe that under-

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standing the role of innovation is crucial to understanding what has happened and what needs to be done.

The Role of Innovation

The analyses conducted in the 1950s of economic growth were built on the idea that saving and capital investment are the engines of growth. Moreover, economic policymaking in the postwar era has been predicated on the assumption that productivity is enhanced primarily by new plants and equipment. Evidence has accumulated, however, that new technology may be an even more important source of growth. New products and new methods of production are the origin of or the catalyst for productivity advance. For example, Brookings economist Edward Denison finds that only 20 percent of the increase in output per employee over the period 1929–82 was the result purely of increased capital per worker.[5] Innovation contributes to growth as new ideas are applied directly to improve productive efficiency and as new technology is incorporated into new equipment and new plants. Indeed, without the flow of new technology, capital spending itself tends to dry up. The expansion of capital, as an end in itself, is only a minor motive for investment; the main motive is that new equipment and new technology are purchased together.

Innovation is just as important to U.S. foreign trade. The National Science Foundation has charted with startling clarity (see Figure 1) the extent to which the United States became a net exporter of high-technology products and a net importer of other products in the 1970s. This theme has been reinforced by Brookings author Robert Lawrence, who has also found that high-tech industries have weathered the effects of the overvaluation of the dollar in the 1980s better than other industries.[6]

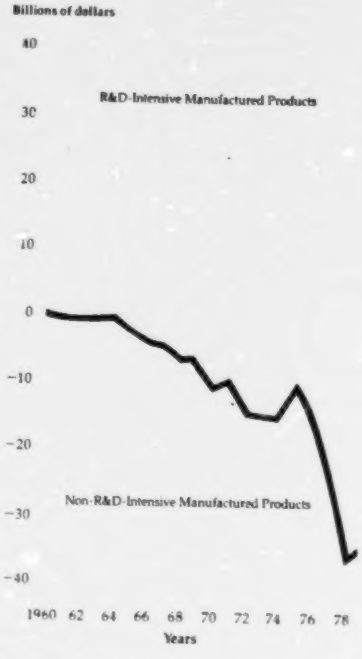
The Slowing of Innovation

There are two important hints that the pace of innovation in the United States has slowed down. The first of these has also been charted by the National Science Foundation (see Figure 2): Patents granted to U.S. inventors peaked in 1972 and declined substantially over the next ten years.

The second reason to think that innovation has slowed is that productivity growth in the majority of U.S. manufacturing industries declined after 1973. Since innovation is a key source of productivity growth, a slowing of the pace of innovation is a likely cause of slow productivity growth. In our own work, we have been looking at precisely this link between innovation and productivity. We analyzed two manufacturing industries: the chemical industry, which had a substantial productivity slowdown in the 1970s, and the textile industry, which did not. We collected information on the flow of innovation in each industry and found a marked relation between innovation and productivity trends: The industry in which productivity slowed—chemicals—was also the industry in which innovation slowed. Figure 3 illustrates this (see page 17).

Since the majority of industries in U.S. manufacturing (and many others, too) had productivity experiences more like that in chemicals than that in textiles, these findings support the view that innovation slowed in the 1970s and pulled productivity growth down with it.

Figure 1.
U.S. Trade Balance in R&D-Intensive and Non-R&D-Intensive Manufactured Product Groups

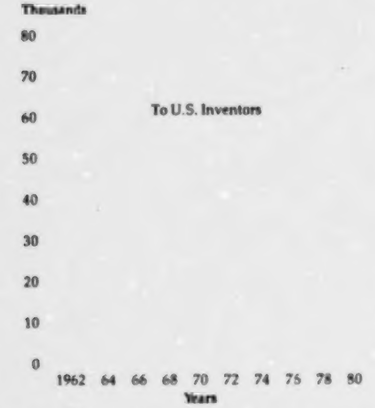


Source: National Science Board, *Science Indicators 1980*. Government Printing Office, 1981, p. 32 (Figure 1-12).

Reasons for Weak Innovation

The slackening of innovation is due in part to the maturing of technology in many fields.[7] The Great Depression of the 1930s disrupted the economy, deprived the business community of resources necessary for development, and discouraged risk-taking. World War II created a forced-draft of scientific and technological development, but with applications geared to war production. As a result, the postwar period was marked by tremendous opportunities for the commercial development of technology, and these oppor-

Figure 2.
Patents Granted to U.S. Inventors, 1960–82



Source: National Science Board, *Science Indicators 1982*, Government Printing Office, 1983, p. 101 (Figure 4-8).

> *"Since the majority of industries in U.S. manufacturing (and many others, too) had productivity experiences more like that in chemicals than that in textiles, these findings support the view that innovation slowed in the 1970s and pulled productivity growth down with it."*

tunities fueled innovation and productivity growth for two decades.

However, the natural maturing of technology is not an entirely satisfactory explanation for weak innovation. Indeed, the maturing of one direction for progress could have served as a challenge to firms to chart other directions. But in fact this did not happen often enough. Companies decided that the return to technological development had fallen, and they reduced their commitment to it. Rising inflation, together with tax provisions allowing all interest payments to be deducted from income, made it more attractive to purchase safe assets that could be highly leveraged—and decreased the appeal of risky investment in new technology. One sign of this is that company-financed R&D spending grew no faster than GNP over the period 1967–77.[8] There seems to be a natural law that R&D spending has to grow faster than output if the pace of innovation is to be kept up.[9] When this acceleration did not materialize in the early 1970s, the pace of innovation fell.

In fairness to the business community, it should be said that external factors played an important role in this decline. Companies had to deal with inflation, deep recession, sharply increased energy costs, and proliferating environmental and worker-safety regulations. The squeeze on R&D budgets came from a squeeze on profits. And technical and managerial resources were diverted away from their normal tasks to deal with energy or environmental problems. Beginning in 1977–78, R&D spending increased rapidly again as companies realized the importance of new technology to growth and profitability.

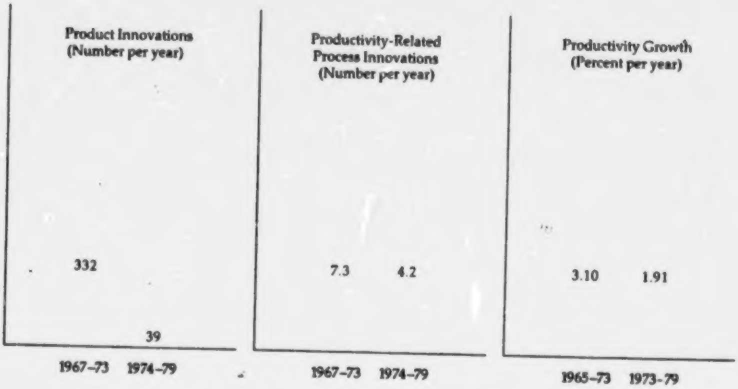
Why Innovation in Electronics Has Not Been Enough

We have said that the United States has been going through a period of weak innovation. That may seem an odd assertion, given the explosion of new computer and electronic innovations in recent years. Indeed, despite fierce competition in consumer electronics and computer chips, the United States remains the overall technology leader in electronics.

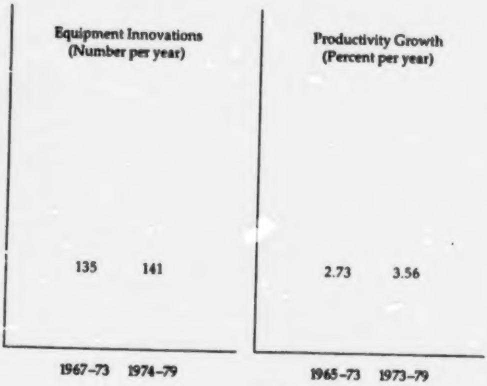
The prospects for U.S. industry would be much bleaker were it not for the success of the electronics industry, and we do not wish to minimize its achievements. But innovation in this industry has not been enough to offset the slowdown elsewhere. The puzzling thing is that the computer revolution has not yet paid off to the same extent as did the earlier generations of innovation. Much of the capital investment made in the United States in the 1970s was for computer and electronic equipment. Such equipment made up 21 percent of all purchases of nonresidential durable equipment in 1973, a figure that had risen to 44 percent by 1983.[10] And yet most of the industries making this investment experienced slow productivity growth. In fact, the fraction of total employment that consists of white-collar workers continued to rise in the 1970s, as did the fraction of clerical workers.[11] Within all the major industry groups of the economy, the ratio of overhead or non-production worker employment to total employment also continued to rise in the 1970s.[12] It looks as if the administrative bureaucracies of the economy swallowed a large share of total investment without making corresponding efficiency improvements.

There is one optimistic explanation of the pattern that we

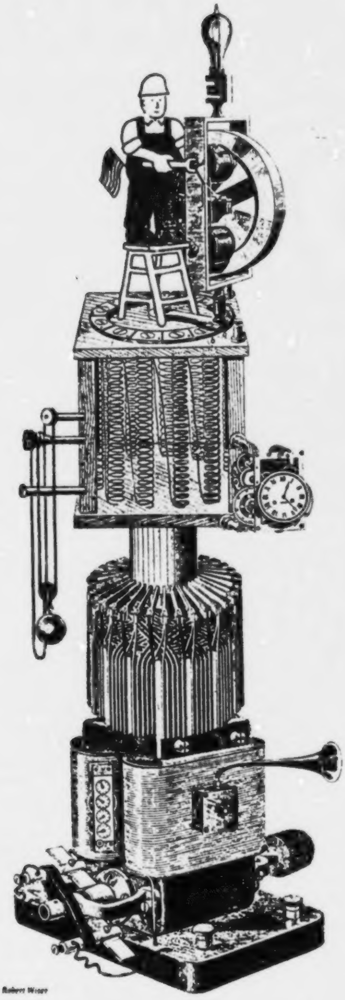
Figure 3.
Innovation and productivity both slowed in the chemical industry. . .



but not in the textile industry



Source: Martin Neil Baily and Alok K. Chakrabarti, "Innovation and Productivity in U.S. Industry," *Brookings Papers on Economic Activity,* 2: 1985 (forthcoming).



Robert Wiser

have just described. It could be that the impact of computerization has been to improve the quality of products and services provided and that this improvement is not being picked up. The problem could be in the measurement of productivity, not in productivity itself. And in some areas, this optimistic view must be correct. For example, in financial services a broad range of new products have been offered. The credit card revolution could not have taken place without computerization, and consumers benefit greatly from the resulting convenience.

There is an alternative and more pessimistic explanation, however. Office automation has differed fundamentally from factory automation. When a factory is automated the same output can be produced with less labor, or more output can be produced with the same amount of labor. The company automating has two choices: either to cut employment or to cut prices and expand sales. Either way, productivity rises. When the office is automated there is a third option, which is simply to generate more paper. Each document goes through four drafts instead of two. The accountants simulate the tax consequences of a variety of corporate strategies. The marketing department makes a dozen computer projections based on demographic changes and migration patterns and so on. In addition, the incentives may be wrong. Middle managers are reluctant to share secretaries or reduce the number of people they supervise, so there is persistent overstaffing.

The government has added to the number of ways in which computers are used that may not be fully productive. The tax law has become extremely complex, keeping accounting departments busy and their employment high. And companies must now monitor a whole range of production and employment activities to make sure that they conform to regulatory requirements.

One sign of the importance of this issue is that when electronics has been applied directly to the production of final outputs, it has brought about amazing productivity gains. The telephone communications industry — in which electronic equipment provides service directly to customers — is a good example. This industry achieved a very high rate of productivity growth, and sustained it throughout the 1970s. Another illustration of the same point is the automated teller machine, which has dramatically boosted productivity in the provision of personal banking services.

The more common case, however, is one in which the new technology has been applied to support areas such as accounting, marketing, and personnel.[13] And it is here that productivity gains may not have been realized, at least not yet.

What Is to Be Done: Business Strategies

Perhaps the most important lesson that comes out of our work on innovation is the importance of viewing technology on a worldwide basis. The United States is no longer alone on the frontier of technology, and the implications of this change have not been fully absorbed. A senior R&D officer at a leading U.S. corporation told us that his company kept a technical listening post in Japan. He was embarrassed about it and tried to take back the information. That is absurd. Of course, his company should be following innovations in Japan. The Japanese have been copying his company's technology for years. His company takes justi-

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fiable pride in being the technological leader in its product lines, but that is no reason to neglect worldwide developments in technology. Japan, Western Europe, and even Eastern Europe are major sources of innovation. The United States has supplied technology to the world for decades, and there is nothing wrong in getting a return flow. As other countries catch up in technology and put more resources into research, it is inevitable that more and more innovations will originate outside the United States. Unless U.S. companies track what is happening overseas, they will be left behind in important areas of technology.

This argument does not mean that U.S. industry should try to copy the Japanese road to success. The Japanese are masters at adapting and improving technology and then producing items at low cost. The U.S. comparative advantage is still in advanced technology. The challenge for America is to keep up with the best from overseas and then take another step forward.

The recent fortunes of the American textile-machinery industry illustrate the perils of ignoring foreign innovations. U.S.-built textile machinery was the best in the world for years, but in the 1960s the Swiss and Japanese developed a new shuttle-less technology that offered tremendous cost and productivity benefits. This advance was quickly adopted by European and Japanese manufacturers other than those who had pioneered the development. But the U.S. industry stuck to the task of perfecting the old technology. Most of the spinning and weaving machinery purchased by the U.S. textile industry is now imported.

Viewing technology on a worldwide basis strengthens the case for innovation itself. Information about technology now flows very quickly around the world. In the past, a U.S. industry with a superior technology could count on maintaining a worldwide advantage in production cost or product quality for several years. Now an innovative new product can be copied and produced overseas more quickly. Continuous technological improvement has become a necessity for maintaining a competitive position.

The second lesson from our study of innovation and productivity is that, if investment in electronics and information-processing equipment has been rising rapidly while productivity has been weak, then it is time to evaluate closely the return from such investments. When office tasks are automated, the staff requirements for completing these tasks should be reduced. The technology should be used in new ways only if they can be demonstrated to be worthwhile. One possible reason for the small productivity impact of computer technology is that companies are engaged in a long-term learning process. Right now the investment does not pay off, but each company continues to spend in this area because it cannot afford to be left behind. That bodes well for future productivity, provided that U.S. industry does in fact move along the learning curve and finds ways to automate white-collar operations efficiently.

What Is to Be Done: Policy

Since innovation is so important to the U.S. economy, an obvious priority of policy should be to make sure that there are adequate incentives for the development of new technology in the private sector. University of Pennsylvania economist Edwin Mansfield has found that the return to society from industrial innovation is much higher than the return to the innovating company. Competitors quickly copy new products, and key personnel change companies or set up their own companies, taking their knowledge with them. The rapid flow of technological information overseas means that repetitions of new products often come from overseas and further erode the returns to U.S. innovators. This diffusion of knowledge benefits consumers because the competition that it creates helps to bring prices down. But the consequently short half-life of a technological edge in the modern world leads to underinvestment in R&D. The private market fails to provide an adequate incentive for innovation.

In these circumstances, it is appropriate for tax policy to give to the private sector an additional stimulus over and above the market incentive. Current tax law does this, as the provisions that allow R&D costs to be expensed and establish an incremental tax credit for R&D spending result in a small positive incentive for R&D. Some economists have expressed concern about the effectiveness of this incentive and about the possibility that companies abuse the tax credit by classifying marketing expenditures as R&D. Indeed, based on these concerns, proposals have been made to reduce or eliminate the current incentive provisions. We believe that *provided* the tax incentives can be properly targeted, they should be retained or even strengthened. Efforts to reduce the budget deficit, which we strongly support, should not eliminate the modest pro-growth provisions now in the tax code.

A second area for policy initiatives concerns steps that the government might take to increase the technological opportunities available to industry. Recent history demonstrates that the government has played a vital role in nurturing high technology in the United States.[14] Programs in the

Defense Department were instrumental in pushing forward the development of early computers, and the aircraft industry continues to be aided by defense money for development. Government's role is so important because there is often a barrier, which private companies cannot overcome, to opening up major new areas of opportunity. Massive resources may be necessary, the risks are extremely high, and once progress has been made, the group that pioneered a development may well not be able to appropriate its benefits.

There is some recognition in Washington of the need to provide an impetus for technological development. The National Science Foundation has shifted its orientation and is increasingly concerned with how scientific development can help the economy. It is setting up centers for "generic" technology development. These try to promote new ways of approaching engineering and scientific problems (e.g., how to minimize friction).

In addition, the link between defense and the development of new technology is cited as part of the case for the Strategic Defense Initiative. The proponents of that program argue that it will help U.S. industry take a giant step forward in computer technology.

Much more needs to be done, however. And the first step should be an evaluation of how the government's role should be structured. The history of government efforts to push forward technology is one of great successes and staggering failures. The atom bomb and the computer were developed; a man was sent to the moon. But the breeder reactor absorbed billions before expiring, and the synfuels project has done no better. The space shuttle is not a disaster, but it has been much more expensive and less reliable than had been hoped.[15]

It is in the nature of the task that spectacular successes and failures will occur. After all, government has a role precisely because of the high risks entailed and the large-scale investments required. But, clearly, an institutional framework that can minimize disastrous losses has not been established. Powerful groups and individuals develop vested interests in continuing projects that are going nowhere. The breeder reactor program lasted more because of its ability to generate jobs in Tennessee than because of its technological potential.

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Economists have long been preoccupied with policies that control the business cycle or improve static efficiency, but only a few focus on policies for growth.[16] The institutional structure of the Federal Reserve Board has been the subject of exhaustive discourse. But the mechanisms for stimulating technological development have remained on the periphery of economic debate. That must be changed.

A third lesson for policy was underscored earlier: It is vital to view technology in worldwide terms. The textile industry again illustrates the points to be made. First, it is important to understand the linkages between this industry and related industries. The basic raw materials for the production of textiles are wool, cotton, and synthetic fibers. U.S. agriculture is immensely productive and sells cotton around the world. The U.S. chemical industry, which produces synthetic fibers, is a technological leader and sells overseas, but its sales are limited by restrictions on fiber imports in many countries. The production of fabrics by spinning and weaving in the United States teeters on the edge of competitiveness. In 1981, the U.S. industry had a small trade surplus, but, as we noted above, it has achieved this position through rapid productivity growth that is largely attributable to innovative foreign-built machinery. Without machinery imports, the U.S. textile industry would not be competitive. Finally, the U.S. apparel industry buys fabric and produces clothing and is not competitive. This industry is labor-intensive, and relatively high U.S. wages push its unit costs above those in developing countries.

How, then, should trade policy be set? Most economists are free-trade purists, and the argument that they make is very strong. The lesson of history and the experience of other countries is that protectionism perpetuates inefficiency and penalizes consumers. Moreover, it is inconsistent for the textile industry to push for protection when the industry is itself dependent on imported machinery.

But free traders are often casual about the costs of their orthodoxy. The textile industry today is bearing the costs of an unsound federal fiscal policy that has led to an overvalued dollar. Moreover, while it can be argued that efficiency would be served by a shrinking of the U.S. apparel industry, such a reduction would also hurt the highly productive U.S. textile fabric and synthetic fiber industries.

On balance, a worldwide perspective on technology strengthens the case for maintaining an open economy. If a foreign country innovates, then U.S. purchasers must have access to the new technology. Quotas, in particular, take away market pressure and reward a reluctance to change. If we are to maintain an open economy, however, U.S. companies must be allowed to compete on fair terms. Fiscal policy that disrupts the normal value of the dollar is simply counterproductive. Moreover, the United States must be willing to threaten trade restraints in order to force other countries to permit U.S.-made goods access to their markets. If the U.S. synthetic fiber and textile fabric industries cannot sell to U.S. apparel manufacturers, they must be allowed to sell to foreign manufacturers that ship clothing to the United States.

In addition, if consumers are to benefit from the flow of cheap foreign goods, then as taxpayers they must also be willing to compensate workers and companies for lost jobs and lost investments. (See the article by Robert Lawrence and Robert Litan in this *Review* for a discussion of these issues.) Temporary tariffs or subsidies may also be appropriate for industries hit by foreign competition at times when the dollar is overvalued. Economic efficiency is not served by wiping out a basically competitive industry because of short-run financial fluctuations.

Conclusions

Innovation is a vital element in the competitive position of the U.S. economy. Traditionally the United States has been the technological leader in most industries and has maintained this status through scientific achievement, government-sponsored technological development, and heavy R&D spending.

Other countries have been catching up to the United States and have overtaken us in some areas. That is not all bad. We can learn from the work of others and accept the fact that not all innovations are made here. But this new order has created difficulties for the U.S. economy, as U.S. industries no longer have a monopoly on the latest technology. Moreover, U.S. innovation appears to have slowed in recent years and pulled down the rate of productivity growth.

Certainly, there is recognition in Washington of the importance of innovation. It has become a catchword in the Reagan White House and among the Atari Democrats. In this essay, we have suggested that (1) technology must be seen on a worldwide basis by both business and government; (2) investments in office automation must be given the same degree of scrutiny as factory automation; (3) tax incentives should be used to sustain industrial R&D in the face of inadequate market incentives; and (4) ways must be found for the government to sponsor major breakthroughs in technology without falling into the breeder-reactor trap.

Notes

1. National Science Board, *Science Indicators 1982*, Government Printing Office, 1983, table 4-11, p. 287.

2. In 1979, the United States had 620,000 scientists and engineers engaged in R&D compared with 477,000 for France, West Germany, and Japan. Expenditures on civilian R&D place the United States a little behind the total of the other three. See *Science Indicators 1982*.

3. H. E. Fitzgibbons, in *High Technology: Public Policy for the 1980s*, issued by the *National Journal*, Jan. 22, 1983, pp. 39-41.

4. The Japanese project is heavily weighted toward basic research, so that our comparison is not a perfect one. However, the Japanese effort itself seems to be failing. According to the *Wall Street Journal*, Sept. 16, 1985, p. 826, "The flow of government money is slowing, participating companies are increasingly reluctant to donate their engineers and important lines of research are being dropped."

5. Edward F. Denison, *Trends in American Economic Growth, 1929-1982*, Brookings, 1985, table 8-3, p. 113.

6. Robert Z. Lawrence, *Can America Compete?*, Brookings, 1984. Recent U.S. trade patterns are described in U.S. Department of Commerce, *United States Trade: Performance in 1984 and Outlook*, Washington, D.C., June, 1985.

7. We do not have a good explanation for why opportunities dried up in some areas but not others. In part, these differences simply reflect the working out of the random process by which technology is developed. One common theme among industries where productivity growth declined is that many of them exploited the gains from large-scale production in the 1950s and '60s. The potential of this approach was exhausted by the 1970s.

8. *Science Indicators 1982*, table 2-2, p. 234.

9. This law has been propounded by F. M. Scherer, now a visiting scholar at Brookings.

10. Robert Z. Lawrence, "The Employment Effect of the New Information Technologies: An Optimistic View," *Brookings Discussion Papers in International Economics*, No. 20, November, 1985.

11. *Employment and Training Report of the President*, Government Printing Office, 1982, table A-19, p. 178.

12. *Ibid.*, table C-2, p. 241.

13. Wassily Leontief and Faye Duchin, *The Impacts of Automation on Employment, 1963-2000*, New York University, April, 1984, pp. 4-12, estimate that only 10 percent of computers are used to control production machinery.

14. See Jerome E. Schnee, "Government Programs and the Growth of High Technology Industries," *Research Policy*, Vol. 7, 1978, pp. 2-24, and Alok K. Chakrabarti and William E. Souder, "Critical Factors in Technological Innovation and Their Policy Implications," *Technovation*, Vol. 2, 1984, pp. 255-275.

15. In a forthcoming Brookings book, Roger Noll and Linda Cohen will examine the political and economic forces behind several government technology programs.

16. One economist who does look at these issues is Richard R. Nelson. See, for example, *High Technology Policies: A Five-Nation Comparison*, American Enterprise Institute, 1984.

DISCUSSION

Mr. **Fuqua**. Thank you very much, Dr. Baily.

There are some who have suggested that a fixed percentage of the GNP would be a way to establish the level of Government science funding, and this would automatically increase funding as the GNP expands and if it didn't expand, then it would reduce the funding. But it also removes some flexibility that maybe the Federal budget situation might require. Have you given any thought to this as an economist?

Dr. **Baily**. Yes, a little. I think that you have posed the dilemma quite correctly, that it reduces the flexibility that you might want to have when you see opportunities. If the opportunities are particularly strong, it would be a period to perhaps increase the amount of relative—amount of funding relative to GNP. Let me start at the beginning.

I think the advantages of some such rule like that are, first of all, you can compare to other countries. We are all a little bit in the dark. I don't think any of us know the direct dollars and cents payoff from basic research or even quite a lot of applied research. We know from general experience that our economy and our standard of living have been dependent on this kind of development, but we can't say where the margin is; we can't say we are getting too high or too low a rate of return, so we are going to have to operate on some kind of rule-of-thumb, and this does allow us to compare with other countries. It does provide some kind of stability, and I think John Kendrick is going to mention this also.

Having this as a percentage of GNP might avoid some of the swings we have had in the past in terms of funding, and I think that stability is important because research is very much a long-term enterprise; and if scientists and engineers never know and if suddenly there is going to be a big cutback, lots of people are going to be laid off, I think that is destructive.

I would say not an adherence to a total strict rule. I don't think you should say this is rule, and you're never going to change it. I think you should have some kind of percentage rule; and then if there is clear evidence that the opportunities are such that this is a good time to put more funding in, then increase it. If there's clear evidence this is not a good time, and we need to wait for some other things to come along, that would be a time to decrease it. So have a rule and only change it under good reason.

Mr. **Fuqua**. What can we learn as far as some of the methods and mechanisms of funding that takes place in other countries that might be beneficial to the scientific community or to the Government science policy?

Dr. **Baily**. Well, that was touched on a little bit in the previous testimony. The United States has relied much more on peer review, I think, than other countries have. That has been our primary mechanism. I think it would be helpful to have a slightly more diversified approach. The problem with peer review, in my view, is that it tends to favor the conventional wisdom too much, that ideas that are a little bit off beat, ideas that conflict with what the conventional wisdom is, tend to get given too short shrift. I would prefer a much more diversified approach, as mentioned earlier, perhaps funding institutions that have proven to have a good track record and you say to them, "Well, you decide among your young people or among your bright, innovative people, you decide which ones to give some support to. We will support the institution and then you review that every few years and say, you know, you have been wasting your money or not." But to have each project done by peer review may be too much of a constricting influence.

Mr. **Fuqua**. Thank you very much, Dr. Baily.

Dr. **Baily**. Thank you.

Mr. **Fuqua**. We appreciate your sharing with us your thoughts this morning.

We are going to have to take a short break before we take Dr. Kendrick. We will take about a 10-minute break and come back and get to you. There are a lot of activities going on today, and I will be back in just a few minutes.

[Recess.]

Mr. **Fuqua**. Dr. Kendrick, we are pleased to have you join us this—it's still morning. I apologize for the delay. Please proceed. If you wish to summarize your statement, we will make it part of the record.

[A biographical sketch of Dr. Kendrick follows:]

Dr. John W. Kendrick

John W. Kendrick is a professor of Economics at the George Washington University, a postion he has held since 1956 with occasional leaves. He is also an adjunct scholar of the American Enterprise Institute. In 1976-77 he was Chief Economist for the U.S. Department of Commerce, and in 1972-73 was Vice President for Economic Research at the Conference Board. He was an economist with the National resources Planning Board, 1941-43; statistical control officer with the U.S. Air Force, 1943-46; business seconomist with the U.S. Department of Commerce, 1946-53; and a full-time staff member of the NBER, 1953-56, before joining the faculty of G.W.U., and a part-time member, 1956-78. He has served as an economic consultant to industry and government and on advisory committees to the Department of Commerce, Office of Management and Budget, and the National Science Foundation.

Kendrick received his B.A. and M.A. degrees from the University of North Carolina, and a Ph.D. at the George Washington University. He is a fellow of the American statistical Association and the National Association of Business Economists, and a member of Phi Beta Kappa, the American Economic Association, the Southern Economic Association (President, 1982-83), and several other professional associations. He is on the Editorial Board of the *Review of Income and Wealth*. He was president and Chairman of the board of the National Economists Club (1975-77). He is also on the Boards of Directors of the Pioneer Funds of Boston, and the American Productivity Center in Houston. During summer sessions or leaves fron G.W.U. he has taught at Georgetown, Connecticut, U.C.L.A., Stanford, Hawaii and Simon Fraser Universities.

His books include *Productivity Trends in the United States* (NBER, 1961); *Measuring Company Productivity: Handbook With Case Studies* for the Conference Board, with Daniel Creamer (1961; revised edition, 1965); *Economic Accounts and Their Uses* (McGraw Hill, 1972); *The Formation and Stocks of Total Capital* (NBER, 1976), *Understanding Productivity* (John Hopkins University Press, 1977); *Productivity in the United States: Trends and Cycles*, with E. Grossman (J.H.U. Press, 1980); and (with the American Productivity Center) *Improving Company Productivity* (J.H.U. Press, 1984). He is also the author of more than 100 articles in various professional journals and magazines, and has lectured on economic topics, particularly productivity, in many countries of the world.

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STATEMENT OF DR. JOHN W. KENDRICK, PROFESSOR OF ECONOMICS, THE GEORGE WASHINGTON UNIVERSITY, WASHINGTON, DC

Dr. **Kendrick**. Well, since my statement is short and since it wasn't available in advance, I think I will read much of it, if I may. I think it answers many of the questions that you posed in part 7 of the task force agenda. I will be happy to answer any others later.

Theoretically, with perfect knowledge on the part of human decisionmakers, there would be an optimum level of research and development outlays, with due regard to other policies to promote scientific and technological progress over successive time periods, and I give the rule to achieve such an optimum. However, this can only be approximated imperfectly in the real world of incomplete knowledge and the attendant risks and uncertainties, differences between private and social returns, indivisibilities of many types of R&D projects, structural differences among industries in terms of the number of firms and so on.

Studies of past developments and relationships, which importantly affect current expectations and decisions, have resulted in a number of quite significant findings. Analysis of individual firms and industries, as well as macroeconomic studies, indicate that private rates of return on R&D have been high, averaging between 30 and 40 percent, even after allowance for the substantial proportion of unsuccessful projects; it has been estimated about a third of projects are dry holes. Further, social returns have averaged at least twice as much as private returns, although the difference between the two, that is, the external returns vary among projects and industries.

The fact that rates of return on R&D, even after reasonable allowance for uncertainties of outcome, are significantly higher than on tangible capital or even on direct human investments, suggests to some observers that there has been underinvestment in R&D. The policy implications of this view are strengthened by the fact that new knowledge and product and process inventions or improvements, resulting from R&D, increase potential innovations and thus shift private tangible investment demand curves upward, augmenting economic growth.

Cost-reducing innovations from R&D, and the associated tangible and human investments, are the chief elements in the growth of productivity. Cross-sectional interindustry and interfirm comparisons indicate that there is a highly significant correlation between the proportions of revenue devoted to R&D, either directly or indirectly through suppliers of capital goods to an industry, and the rates of increase in productivity. I cite in the footnotes a study I did 3 years ago that documents that.

At the aggregate level of the business economy, it is estimated that at least one-half of the increase in total factor productivity has been due to cost reducing technological advances, and in my table 1 on page 2A that is the line called Advances in Knowledge, and notice that from 1948 to 1973 advances in knowledge are estimated to have contributed 1.4 percentage points to the 2.9 increase in output per labor hour. This is based on work by Edward Denison of the Brookings Institution, although some of these estimates are my own. Some are Denison's.

TABLE 1—SOURCES OF GROWTH IN REAL GROSS PRODUCT: U.S. PRIVATE BUSINESS ECONOMY: SELECTED PERIODS 1948–85 AND PROJECTIONS 1985–2000

	Actual	Actual	Actual	Projected
	1948–73	1973–81	1981–85	1985–2000
Average annual percentage growth rates:				
Real gross product	3.6	2.0	2.9	3.3
Total Factor input	1.7	2.0	2.1	1.9
Labor	.7	1.4	1.8	1.1
Capital	3.6	3.2	2.7	3.5
Real product per unit of labor	2.9	.6	1.1	2.2
Capital/labor substitution	1.0	.6	.3	.8
Total factor productivity	1.9	0	.8	1.4
Percentage point contributions to growth:				
Advances in knowledge	1.4	.7	1.0	1.0
Changes in labor quality	.5	.6	.9	.9
Education and training	.6	.7	.7	.6
Health and safety	.1	.1	.1	.1
Age-sex composition	−.2	−.2	.1	.2
Changes in quality of land	0	−.2	−.2	−.3
Resource reallocations	.4	.1	0	0
Volume changes	.3	−.3	.3	.5
Economics of scale	.4	.2	.3	.3
Capacity Utilization	−.1	−.5	0	.2
Government regulations	0	−.2	−.1	0
Actual/potential efficiency and n.e.c.	−.7	−.7	−1.1	−.7

[1] Preliminary.

Note: The estimates 1948-1985 are based on the recently revised estimates of real gross national product, as published in the 1986 Annual Report of The Council of Economic Advisers.—John W. Kendrick, Feb., 1986.

This doesn't include the effects of associated investments in plant and equipment and education, training and so on, all of which are enhanced by new knowledge and know-how. It should also be noted that the output in productivity estimates inadequately reflect the improving quality of goods and services, although new product development indirectly contributes to productivity growth through the learning curve effect, that is, we all know that productivity increases the most in the early stages of production of new goods and before the industry tends to mature, so it takes a continued succession of new products to keep the rate of productivity growth high.

Changes in the contribution of technological progress to productivity growth can be estimated roughly through estimating the rate of growth of the real stock of knowledge and know-how resulting from R&D, and you get this by cumulating the real R&D investments with allowance for timelags between laboratory work and completed projects and commercial applications.

The slowdown of increases in real R&D and its decline as a ratio to GNP from near 3 percent in 1964 to about 2.2 percent in the mid 1970's resulted in a marked deceleration in the growth of the real R&D stock and increased unemployment of scientists and engineers. I estimate this contributed about 0.7 percentage point to the 2-percent productivity slowdown between 1973 and 1981, and this is in the table. Nestor Terleckyj of the National Planning Association comes up with similar conclusions.

Conversely, the resumption of much stronger growth in real R&D investment beginning in the latter 1970's accounts for about half of the modest acceleration in productivity growth since 1981. We went from 0.6 percent up to about double that in 1981 to 1985.

A prime objective of Federal Government policy regarding total R&D, and its own financing, should be to target steady, relatively predictable annual increases after allowance for price changes. This is desirable in view of the long lead time required for the education and training of scientists, engineers, and other personnel, and between the execution of R&D and its effects on productivity and quality. The mischievous effects of declines in Federal funding and deceleration in total real R&D outlays I have indicated.

In my view a minimum target should be increased in total real R&D investment in line with the growth potential of the U.S. economy, which is estimated by the Labor Department at around 3½ percent between 1985 and 1995. This means that the ratio of R&D to gross national product would be close to 3 percent of the recent ratio in years of high level economic activity. This "rule of thumb" is proposed as a floor in view of the strong evidence of continuing high returns to R&D, including the productivity effects of real unit cost reduction. If strong evidence of underinvestment or overinvestment in R&D were adduced in the future, the target rate of growth could be changed, but gradually, to avoid inflationary competition for limited personnel in the case of increases, or significant increases in unemployment of scientists and engineers in the case of decreases in the growth rate of real R&D performance.

Although the Federal Government share of R&D fell below 50 percent a few years ago in 1980, and business R&D became 51 percent of the total, the Government can still strongly influence the trend in total R&D. In addition to controlling the amount of its own R&D for public purposes, it influences private expenditures in the first instance by the stimulative effect of contract R&D and other federally funded, but privately performed, research and development. I footnote an important report by Nestor Terleckyj in which he indicates that companies and industries doing Government R&D actually do more of their own, and also R&D tax credits and other means of stimulating R&D, can be varied by the Government, and thus stimulate private R&D and narrow the gap between social and private returns, which happens when you are getting more private R&D.

Since externalities and business perception of risks are greater the further removed from development and engineering the R&D is, the Federal support of basic research and intrastructural or generic R&D is particularly important. Interestingly, private funding of such R&D has also grown rapidly in recent years, indicating, I think, a lengthening of the planning horizons of business executives in planning their own investment. This was stressed in the latest report of the National Science Board 1985 Science Indicators report, which is a good, I think, a very good summary of the current state of science.

Policy measures are more difficult to formulate when selective rather than across-the-board support of R&D is called for. This is the case when private rates of return are low, but social returns are high, or when an industry is composed mainly of small firms

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which cannot afford desirable larger R&D projects. The latter situation has recently been ameliorated somewhat by the more permissive antitrust policies of the current administration with respect to mergers, R&D consortia, and joint ventures. To some extent Federal purchasing policies and contract R&D can also be used to try to optimize the total amount and its composition.

The Federal Government also influences the volume of R&D indirectly through fiscal and regulatory policies that affect other types of investment, particularly durable equipment, in which new technology is embodied. More broadly, macroeconomic policies that affect current and prospective rates of economic growth and net business income also have an influence on the amount of business-financed R&D.

Government R&D associated with the production of public goods, including national security, which in my mind is a very important public purpose, are subject to the same criteria as business investment, so the value of the future benefit may be more difficult to calculate, except in the case of cost-reducing projects. If Government outlays and production of public goods were to remain a constant proportion of gross national product—and we seem to have gotten down to a fairly constant ratio recently—one would also expect a relatively constant ratio of the related public R&D to gross national product.

These remarks underscore the complexity of the task of achieving a good approximation to an optimal level and allocation of Federal and other sources of funding for research and development. In my view, the capabilities of the Office of Science and Technology Policy should be strengthened to provide for continuing research on the various facets of the subject, and to provide broad direction and coordination of the relevant activities of the various Federal Government departments and agencies involved in this endeavor. In view of budgetary constraints, special attention should be paid to the possible tradeoffs between Federal R&D funding and other policy measures for promoting scientific and technological progress.

[The prepared statement of Dr. Kendrick follows:]

Committee on Science and Technology

U. S. House of Representatives

Science Policy Task Force

Hearings of April 15-16 on

"National Research Funding Levels"

Statement by John W. Kendrick

The George Washington University

April 15, 1986

National Research Funding Levels

Statement by John W. Kendrick
The George Washington University

Theoretically, with perfect knowledge on the part of human decision makers, there would be an optimum level of research and development (R&D) outlays, in combination with other policies to promote scientific and technological progress in successive time-periods. In effect, R&D investments, private and public, would be carried to the point at which social rates of return were equal to the marginal cost of funds. To state it more precisely, the various types of R&D investments would be undertaken up to the point at which the present value of the social returns (including non-monetary benefits) would equal the costs, private and public.

This ideal can be approximated only imperfectly in the real world of incomplete knowledge and the attendant risks and uncertainties, differences between private and social returns which vary among types of R&D, "indivisibilities or "lumpiness" of many types of R&D projects, structural differences across industries, and other economic sectors, and so on.

Studies of past developments and relationships, which importantly affect current expectations and decisions, have resulted in a variety of significant findings. Analysis of individual firms and industries, as well as macro-economic analysis, indicate that private rates of return on R&D have been high, averaging between 30 and 40 percent even after allowance for the substantial proportion of unsuccessful projects. Further, social returns have averaged at least twice as much as private returns, although the difference between the two, i.e., the "external returns" vary among projects and industries, and are not correlated with private rates of return.[1]

The fact that rates of return on R&D, even after reasonable allowance for uncertainties of outcome, are significantly higher than on tangible capital or even on direct human investments, suggests to some observers that there has been under-investment in research and development. The policy implications of this view are strengthened by the fact that new knowledge and product and process inventions or improvements, resulting from R&D, increase potential innovations and thus shift private tangible investment demand curves upwards, augmenting economic growth.

Cost-reducing innovations in processes, producers' goods, and the organization of production resulting from R&D and the associated tangible and human investment, are the chief element in productivity growth. Cross-sectional interindustry and inter-firm multiple regressions indicate that there is a highly significant correlation between the proportions of revenue devoted to R&D, either directly or indirectly through suppliers, and the rates of increase in productivity.[2]

At the aggregate level of the business economy, it is estimated that at least one-half of the increase in total factor productivity has been due to cost-reducing technological advances (see Table 1) although not all of this stems directly from research and development. This does not include the effects of the associated investments in plant and equipment, nor in the education, training, health, safety and mobility of workers, all of which are enhanced by new knowledge and know-how. It should also be noted that the output and productivity estimates inadequately reflect the improving quality of goods and services, although new product development indirectly contributes to productivity growth through the learning curve effect.

TABLE 1.

Sources of Growth in Real Gross Product: U. S. Private Business Economy:

Selected Periods 1948-85 and Projections 1985-2000.

	Actual			Projected
	1948-73	1973-81	1981-85^P	1985-2000
	(Average annual percentage growth rates)			
Real gross product	3.6	2.0	2.9	3.3
Total factor input	1.7	2.0	2.1	1.9
Labor	0.7	1.4	1.8	1.1
Capital	3.6	3.2	2.7	3.5
Real product per unit of labor	2.9	0.6	1.1	2.2
Capital/labor substitution	1.0	0.6	0.3	0.8
Total factor productivity	1.9	-0-	0.8	1.4
	(Percentage point contributions to growth)			
Advances in knowledge	1.4	0.7	1.0	1.0
Changes in labor quality	0.5	0.6	0.9	0.9
Education and training	0.6	0.7	0.7	0.6
Health and safety	0.1	0.1	0.1	0.1
Age-sex composition	-0.2	-0.2	0.1	0.2
Changes in quality of land	0	-0.2	-0.2	-0.3
Resource reallocations	0.4	0.1	0	0
Volume changes	0.3	-0.3	0.3	0.5
Economies of scale	0.4	0.2	0.3	0.3
Capacity Utilization	-0.1	-0.5	0	0.2
Government regulations	0	-0.2	-0.1	0
Actual/potential efficiency and n.e.c.	-0.7	-0.7	-1.1	-0.7

P = preliminary

Note: The estimates 1948-85 are based on the recently revised estimates of real gross national product, as published in the 1986 Annual Report of The Council of Economic Advisers.

John W. Kendrick
Feb., 1986

Changes in the contribution of technological progress to productivity growth can be estimated roughly by reference to changes in the rate of growth of the real stock of knowledge and know-how resulting from research and development. The real stock is obtained by cumulating real R&D investment with allowance for the time-lags between laboratory work and completed projects, commercial application and diffusion of the resulting innovations. The slowdown of increases in real R&D, and the decline in its ratio to gross national product (GNP) from near 3% in 1964 to about 2.2% in 1978 resulted in a marked deceleration in the growth of the real R&D stock, and increased unemployment of scientists and engineers. I estimate that this contributed about 0.7 percentage point to the productivity slowdown between 1973 and 1981. This compares with Nestor Terleckyj's estimate that the deceleration in growth of government financed real R&D directly and indirectly was responsible for 0.55 percentage point of the productivity slow-down.[3] Conversely, the resumption of much stronger growth in real R&D investment beginning in the latter 1970s accounts for about half of the modest acceleration in productivity growth since 1981 (see Table 1).

A prime objective of Federal Government policy regarding total R&D, and its own funding, should be to target steady, relatively predictable annual increases after allowance for price changes. This is desirable in view of the long lead times required for the education and training of scientists, engineers, and supporting personnel, and between the execution of R&D and its effects on productivity and quality. The mischievous effects of declines in Federal funding, and deceleration in total real R&D outlays were indicated above. In my view, a minimum target should be increases in total real R&D investment in line with the growth potential of the U. S. economy, which is estimated at around 3½% a year over the next decade.[4] This would mean that the ratio of R&D to GNP would be close to 3% in years of high-level economic activity, and somewhat more in years of recession or slowing of expansion. This "rule of thumb" is proposed as a floor, in view of the strong evidence of continuing high return to R&D, including the productivity effects of real unit cost reduction. If strong evidence of under-investment or over-investment in R&D were adduced in the future, the target rate of growth could be increased or decreased. Changes in growth rates should be gradual, however, to avoid inflationary competition for limited scientific and technological resources in the case of increases, or significant increases in unemployment of valuable talent, as in the early 1970s.

Although the Federal Government share of R&D funding fell below 50% a few years ago, it can still strongly influence the trend in total R&D expenditures. In addition to controlling the amount of its own R&D for public purposes, it influences private expenditures in the first instance by the stimulation effect of contract R&D and other Federally funded but privately performed research and development.[5] In addition, the measures such as permitting the expensing of business R&D, and R&D tax credits, which may be varied in amount, have been effective in increasing private R&D by reducing risks and unit costs, thus narrowing the average gap between social and private returns.

Since externalities and business perception of risks are greater the further removed from development and engineering the R&D is, Federal support of basic research and infra-structural or generic R&D is particularly important. Interestingly, private funding of such research has also grown rapidly in recent years, indicating a lengthening of planning horizons against which business executives view investment.

Policy measures are more difficult to formulate when selective rather than across-the-board support of R&D projects is called for. This is the case when private rates of return are low, but social returns are high, or when an industry is composed mainly of small firms which cannot afford desirable R&D projects. The latter situation has recently been ameliorated somewhat by the more permissive anti-trust policies of the current administration with respect to mergers, R&D consortia and joint ventures. To some extent Federal purchasing policies and contract R&D can also be used in a selective manner.

The Federal Government also influences the volume of R&D indirectly through fiscal and regulatory policies that affect other types of investment, particularly producers durable equipment which is a prime carrier of new technology. More broadly, macro-economic policies that affect current and prospective rates of economic expansion and net business income also have an influence on business-financed research and development.

Government R&D associated with the production of public goods, including national security, are subject to the same criteria as business investment, though the value of the future benefits may be more difficult to calculate except inthe case of cost-reducing projects. If government outlays and production of public goods were to remain a constant proportion of GNP, one would also expect a relatively constant ratio of the related R&D to GNP, unless there were good evidence that this ratio should be adjusted.

These remarks underscore the complexity of the task of achieving a good approximation to an optimal level and allocation of Federal and other sources of funding of research and development. In my view, the capabilities of the Office of Science and Technology Policy should be strengthened to provide for continuing research on the various facets of the subject, and to provide broad direction and coordination of the relevant activities of the various Federal Government departments and agencies involved. In view of budgetary constraints, special attention should be paid to the possible trade-offs between Federal R&D funding and other policy measures for promoting scientific and technological progress.

FOOTNOTES

[1] Most of the research findings cited in this paper are referenced in two reports: Nestor E. Terleckyj, Economic Effects of Government R&D Spending on the United States, Final Report to the National Science Foundation on the Project PRA 81-16098 (Washington: National Planning Association, August 31, 1984); and Relationships Between R&D and Economic Growth/Productivity, Preliminary Papers for a Colloquium (Washington: National Science Foundation, November 9, 1977).

[2] See John W. Kendrick, Interindustry Differences in Productivity Growth (Washington: American Enterprise Institute, 1983), pp. 42-50.

[3] See Terleckyj, op. cit., pp. 113-115.

[4] Projections of U. S. economic growth 1985-1995 prepared by the Bureau of Labor Statistics are published in the November 1985 Monthly Labor Review. See also John W. Kendrick, "Long-Term Economic Projection: Stronger U. S. Growth Ahead," Southern Economic Journal, April 1984.

[5] The stimulative effects of Federal Government R&D on private R&D investment are documented in Terleckyj, op. cit., Chapter 2, pp. 19-33.

DISCUSSION

Mr. **Fuqua**. Thank you very much, Dr. Kendrick.

In your closing remarks you referred to the Office of Science and Technology Policy should be strengthened by the continuing research on various facets of the subject by broad direction and coordination. Could you be a little more specific in what you are referring to?

Dr. **Kendrick**. Yes. The research I am thinking of establishing would establish the sorts of findings I referred to; that is, what are the rates of return in different industries and types of research? People like Griliches of Harvard and Mansfield of Pennsylvania and Terleckyj here in Washington have estimated rates of return, both private and social, on different kinds of R&D. I think that should be monitored year by year. What are the rates we are getting ex-post because of decisions by businessmen as to the conduct of the future depends on what they think they have been getting in the past, because they tend to project the past experience? And, with respect to coordination of the R&D in different agencies, to see whether our decentralized system is giving us a good allocation of R&D resources among different kinds of research.

With regard to one of the questions in your agenda, I think it would be foolish to try to maintain preeminence in all industrial areas, for example, or other areas like health and safety and so on. In the first place, we couldn't do it given the scientific resources of the rest of the world. Some division of labor is necessary among countries, and I think Baily's idea, with regard to sharing information more broadly with friendly countries, is desirable. To work out such a mechanism, and also some division of labor within the country, we obviously would concentrate on areas in which we have comparative advantage in research and development, personnel and so forth. So it's that sort of coordination that I think the Science Advisor to the President and his Office would try to perform, at least through moral suasion. I don't know how much power they have over the ultimate budget. Certainly OMB, as well, would be involved in deciding on those allocations of funds.

Mr. **Fuqua**. Hasn't that been part of the responsibility and really the experience that OSTP has exercised in the past?

Dr. **Kendrick**. Yes, but they have a relatively small staff. I don't think they have done a lot of continuing research on the sorts of things that would bear on what is the optimal total level, or the optimal allocation. You can only get at that very roughly. I think we need more and continuing work year by year.

Mr. **Fuqua**. What are your thoughts on trying to gear R&D to a basic percentage of GNP? I notice it is used as a barometer in many cases, but it has never been really tied to that.

Dr. **Kendrick**. No. However, it is coming back toward the peak ratio of 3 percent that we reached in the mid-1960's based on decentralized decisionmaking, and the fact that it has been increasing fairly steadily in the last decade makes me feel that we probably had been underinvesting; and also the fact that business is probably taking a longer view under the pressures of international competition. I think that using that ratio as a floor would prevent the kinds of drops in the ratio we had in the late 1960's and early 1970's which had perverse effects and undesirable effects.

But, as I indicated, we could certainly increase the ratio if the kinds of studies I mentioned indicate continuing underinvestment, because if we are getting 30 percent from R&D and 12 percent on plant and equipment, obviously we would do better to conduct more R&D relative to the brick and mortar and machines.

Mr. FUQUA. Getting back to the measuring signs of R&D in science policy, some have said that Vannevar Bush's report "Science: the Endless Frontier" was misnamed; it should have been "Science—The Endless Expenditure." And is there a better method or is there a method, as an economist, to measure the value of science? You talk about OSTP trying to put some measure on it. Some of it is rather intangible, is it not?

Dr. KENDRICK. That is true.

Mr. FUQUA. Particularly in basic research, particularly at universities.

Dr. KENDRICK. Yes. You can measure the benefits more and more as you come up to the new processes and new products. You can get at the value of the new processes and the new producers' goods that cut costs, because you can estimate the present value of that cost reduction and place a value on the inventions that were applied commercially. It's harder to estimate the value of new consumer goods, for example, because you can't really measure the changes in quality of consumer goods and our GNP understates, I think, the growth of output of productivity in the United States by not having an adequate measure of quality changes.

Mr. FUQUA. How do you evaluate or put a value on one of the primary roles of universities, particularly research universities, in the fact that they are training future scientists? Is there any value that you can place on that? Yet we know it's valuable.

Dr. KENDRICK. I think you can place a value on it. You can estimate the money value of a man or woman in terms of their future earning capacity. The training in science and engineering certainly increases the earning power of individuals, and you can put a money value on that. I think the combination of research and of education is very important. As a professor, I feel I bring more to my students through doing research and keeping them abreast of advances in knowledge in my field than if I were not engaged in research, and some of my own research in economics has been supported by the National Science Foundation and other private foundations. So I feel that increases the quality of the education that the young people are getting as well as giving us the human resource pool for future invention and innovation.

Mr. FUQUA. There are some private groups, Chase Econometrics comes to mind, that upon occasion have done studies and put some type of value on the overall return of Federal expenditures. I know they did this on the space program several years ago, approximately 10 years ago. I don't know if that has been updated or not. But are there other groups that have models such as this that can put them together to try to give a better understanding to the public of the value of some of the research that is going on?

Dr. **Kendrick**. Yes, there have been studies, particularly in the aerospace industry which has been heavily supported by government R&D funding.

Dr. **Fuqua**. But this is more applied.

Dr. **Kendrick**. Yes.

Mr. **Fuqua**. I am really trying to get back—that's important, but I am really trying to get back to basic research where the professor at Georgetown or George Washington or some other university is back in a laboratory mixing chemicals together with some graduate students.

Dr. **Kendrick**. I don't know of models, but I know there are a lot of studies that have traced the development of ideas going back to the basic, and tracing them down through application and commercialization and so forth, to see how the past development took place. Professor Rosenberg of Stanford has done a lot of those case studies and so on. Also let me call your attention——

Mr. **Fuqua**. How do you treat a research project that maybe remains dormant, say, for 20 or 50 years before somebody comes along and reads in that literature and a lightbulb goes on and says, "This is what I have been looking for to help solve this problem?" Maybe it was not deemed at that time to really have any material value.

Dr. **Kendrick**. There are a lot of cases of that sort, but I think that now with the information retrieval system made possible through computers that the productivity of research is rising in that we can conduct search and we can do experiments a lot faster than previously. The report by Terleckyj that I cited, footnote one, it is an important one called Economic Effects of Government R&D Spending on the United States, and there is a good deal of research results reported in this report I would like to refer your staff to.

Mr. **Fuqua**. Good.

Dr. Kendrick, thank you very much for waiting. I apologize again for the inconvenience. You have been very patient, and we thank you very much for sharing with us your very informed views this afternoon.

[Whereupon, at 12:17 p.m., the task force was adjourned to reconvene on Wednesday, April 16, 1986.]

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NATIONAL RESEARCH FUNDING LEVELS

WEDNESDAY, APRIL 16, 1986

House of Representatives,
Committee on Science and Technology,
Task Force on Science Policy,
Washington, DC.

The task force met, pursuant to call, at 10:15 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the task force) presiding.

Mr. **Fuqua**. This is the second day of our Science Policy Task Force hearings on national research funding levels. Yesterday we reviewed the science funding levels and priorities for scientific research, and today we're going to explore the strengths of U.S. science in comparison with other nations in trends over time. This is very important as we judge progress in science relative to funds spent.

In the period we're in now, with limited funds available, it is extremely important to know what we mean by national strength in science and how we can measure that strength.

This morning we want to welcome Dr. Nicholas Rescher, university professor of philosophy at the University of Pittsburgh. Also, following him will be Dr. Eugene Garfield, president, Institute for Scientific Information; and following him will be Dr. Christopher Hill, senior specialist in science and technology policy at the Library of Congress.

Dr. Rescher, we'd be pleased to hear from you at this time.

[A biographical sketch of Dr. Rescher follows:]

Dr. Nicholas Rescher

Dr. Nicholas Rescher, is University Professor of Philosophy at the University of Pittsburgh and is also the Director of the Center for Philosophy of Science there. He is the author of *Scientific Progress* (Oxford, 1978) and of *The Limits of Science* (University of California Press, 1985). In 1983, he received the Alexander von Humboldt Research Prize, awarded under the auspices of the Federal Republic of Germany "in recognition of the research accomplishments of humanistic scholars of international distinction."

STATEMENT OF DR. NICHOLAS RESCHER, UNIVERSITY PROFESSOR OF PHILOSOPHY, AND DIRECTOR, CENTER FOR PHILOSOPHY OF SCIENCE, UNIVERSITY OF PITTSBURGH, PITTSBURGH, PA

Dr. **Rescher**. Thank you very much.

Chairman Fuqua, members of the task force, ladies and gentlemen, I very much appreciate this opportunity to appear before you.

The charge that was given to me in the invitation to testify before you invited comments particularly on two issues: First, methodologies for measuring progress and assessing the strength of science; and, second, for more general views regarding the future of science. I propose to address myself to those two issues.

But before I do so, I would like to take this opportunity to applaud the committee and, through it, the Congress for its concern with these very fundamental long-range matters. There are many pressing issues, I'm sure, that dwell in upon you, but this long-range concern does seem to me to be something very crucial for the long-range welfare of the country.

The strength of science can be measured either in terms of the input of resources into scientific work or in terms of output of results from it, and rather distinct things are at issue here.

Input is a matter of viewing science as something akin to an industrial enterprise. From this standpoint, one considers the number of scientific workers, the number and strength of scientific facilities, laboratories, observatories, institutes, and the like, the scale and sophistication of their equipment, and that sort of thing. In the final analysis, such matters can be measured in budgetary terms in terms of the investment of money.

The output of scientific work is something else again. It has to be assessed in terms of its results. It represents the products of scientific efforts by way of publications, procedures, technological devices, and the sort of contrivances and inventions that are the result of scientific work.

This output can itself be assessed in either of two ways: First, quantitatively, in terms of simply the number of experiments of a given sort, observations, calculations, and findings, and in large measure that sort of thing can be tracked by counting scientific publications; or, on the other hand, can be assessed qualitatively, in terms of the significance of the findings, and what matters here are the big discoveries, the important breakthroughs in understanding and in our technological mastery over nature. Here we have to do not with the quantity of results but with their importance.

The quality of scientific work is something that's much harder to measure than its sheer quantity. Difficult, but surely not impossible.

The contributions to science made by a group of scientists, be it those of a single institute or of an entire nation, can be assessed both by objective and by subjective criteria. Let me say a very brief word about each.

Subjective criteria are a matter of how the results achieved by scientists are perceived in the beholding eyes of their colleagues. Peer-group recognition is the key here, with things like Nobel prizes, or honorary degrees, or invitations to major international conferences all serving as indicators. The academic marketplace is also a useful source here: The ways in which students flock to teachers and to problem areas tend to reflect "where the action is." Beyond question, citation indexing is the most sophisticated of these subjective criteria that reflect peer evaluation. However, that's a theme I don't propose to develop further here at the moment. I'll be glad to return to it in discussion later on.

Let me turn to objective criteria. These, by contrast, reflect the applications and technological implications of science. Here the construction of more powerful instruments, the introduction of new products and methodologies, and performance-oriented data, such as the speed of problem solving and computations or the lowering of death rates from disease, would provide indicators. In the final analysis, the efficacy of its applications constitute the best objective indicator of scientific strength and progress.

Let me now turn briefly to the relationship between output and input in scientific work. Viewed in quantitative terms, the relationship between input into scientific work and its output will be roughly linear; that is, a straightforward proportionality obtains. In general, if one increases expenditures by two, one can secure twice as many workers, facilities, publications, and the like.

But all the available evidence suggests that the relationship between input and output when the latter—that is, output—is construed qualitatively is definitely nonlinear. Performance in science is not related to cost by a relationship of simple proportionality. Progress in the way scientific findings at the level of really significant results come about is something that's going to prove exceedingly expensive.

Consider a rather crude analogy. To buy a vehicle that performs twice as well as the average car, one has to spend considerably more than twice as much. In science we confront a situation of diminishing returns where further improvements involve sharply increasing marginal costs.

The reason for this is not far to seek. Progress in natural science depends critically on the availability of an increasingly powerful and sophisticated technology of observation and experimentation. The progress of any given level of performance—I'm sorry—the potential of any given level of performance here is soon exhausted, and one must move on to an increasingly effective and expensive new state for the technological art. The history of telescopes, of particle accelerators, or chemical analysers shows that in science, as in war, we cannot win the battles of the present with the armaments of the past. We're involved in a technological arms race against nature where the demands of enhanced performance commit us to a technology of investigation that's increasingly sophisticated and increasingly expensive.

Just what sorts of returns can we expect to get for these increasing expenditures? That question isn't as simple as it sounds. Before dealing with it, let me back up a little and review some fundamentals.

Perhaps the most basic of these is the fact that scientific progress is unpredictable at the level of detail. We cannot tell in advance what the specific answers to our scientific questions will be. It would, after all, be quite unreasonable to expect detailed prognostications about the outcome—about the particular content of scientific discoveries or about the outcomes of particular experiments. If we could predict discoveries or experiments in detail in advance of the fact, then we could of course make those discoveries in advance or regard those experiments as simply illustrative rather than significant.

In may be possible in some cases to speculate that science will solve a certain problem, but how it will do so lies beyond the ken of those who antedate the discovery itself. In matters of scientific importance then, we must be prepared for surprises, and this is true not only in basic science but of course to some extent in applied science as well.

Commenting shortly after the publication of Frederick Soddy's speculations about atomic bombs in his 1930 book *Science and Life*, Robert A. Millikan, a Nobel laureate in physics, wrote that, "The new evidence born of further scientific study is to the effect that it is highly improbable that there is any appreciable amount of available subatomic energy to tap." In science forecasting, the record of even the most qualified practitioners is poor.

The substance of future science inevitably lies beyond our present grasp. Important scientific innovations are simply, by nature, unpredictable, and the more important, the more unpredictable. In fact, ironically, that's exactly what makes them important, that they could not and were not foreseen, could not be and were not foreseen. Revolutionary discoveries like relativity theory, quantum theory, big-bang cosmology, or double helix genetics are revolutionary precisely because they're fundamentally unforeseeable. The scientific future is inaccessible to even the ablest of present-day workers, and given that it's effectively impossible to predict the details of what future science will accomplish, it's no less impossible to predict in detail what future science will not accomplish.

Now that fundamental fact has far-reaching implications. The circumstance that one cannot predict particular scientific innovations and findings means that we just don't know how to predict scientific discoveries at the level of detail. We're often in a position to know that further scientific effort will pay off in a certain broad area, but we cannot in advance of the fact pinpoint just where; we're bound to be surprised.

Scientific exploration, like geographic exploration, is characterized by unlooked-for findings. Ferdinand and Isabella, by funding Columbus, did not get a short cut to India, but they did launch a vast empire for Spain. Antarctic exploration has not yielded hoped-for returns in ores and minerals, but has greatly contributed to seismology and long-range weather forecasting.

Science management is something akin to what aerial bombardment was like in the Second World War. It would be nice to achieve the precision of going for particular preselected and predesignated targets, but the resources at our disposal then, at any rate, didn't allow it. In natural science, it's only by working across rather broad areas that we can be confident of a payoff. Short of making the effort, we cannot tell just where those weak spots are where nature will yield further discoveries as we press her harder in scientific inquiry.

So there's little point in being overly mission-oriented, in seeking to identify particular problems in basic science that it would be nice to solve, and then throwing money and resources at them. The state of the art is frequently just not ripe for the resolution of particular scientific issues. A far more promising approach is to identify fruitful areas where new methods or instrumentalities open up new opportunities. The time has to be ripe in terms of knowledge already achieved for further developments to be realized. The Einstein proposal reached President Roosevelt at just the right time. A decade earlier, it would have been totally premature. That, too, I think, is a lesson of the Millikan episode I mentioned before.

The wise management of scientific resources calls for a strategy based on the exploitation of the new opportunities afforded by methods or instruments rather than one based on identifying particular problems and addressing them simply by whatever means currently lie to hand. It certainly makes sense in applied science to target the specific technological implementation of existing theory. That presumably is what the Strategic Defense Initiative is about. But in basic science, the more broad-brush approach is called for. Since details are predictably unmanageable, we simply have to proceed on a broader front.

The need for steadiness and continuity of institutional stability and tradition in the prosecution of basic science should also be recognized in this connection. One can count on the fingers of almost one or two hands the institutes in which the double helix model and the cracking of the genetic code at that historical juncture could have been developed. Where such strong foci of capability exist, they deserve support and encouragement and, of course, the necessary freedom of operation to shift effort to where the opportunities of the moment indicate its profitable application.

Even though the relationship between investment in scientific work and the substantive strength of science is, as I've suggested, a very complicated one, the fact remains that there of course is a connection here. The more one invests in a given branch of science, the more substantive progress one can expect to make within it. The more amply one endows its research centers with manpower, equipment, and the support of resources required, the more advances one can expect. What the nonlinearity of the relationship between investment and return means, however, is that in order to maintain even a modest edge over the science of a rival center or nation, one must make a substantially larger investment. That's the sad and ironic fact. To stay ahead in basic science is not just somewhat expensive but very expensive.

One consequence of this is that not even a very affluent nation can expect to keep its science ahead of the competition all across the board. The more it tries for breadth and universality, the more it must expect that a competitor that targets its resources more narrowly in particular areas will be able to outstrip it in those limited chosen fields. Scientific competition is a bit like international sporting competition in this respect. Not even a major power can expect to win every Olympic event. One can't expect to be first all across the board. The difference is that in science, unlike sport, one is in a pretty strong position to select those particular areas in which one is going to prevail. But, to repeat, that's a matter, it seems to me, of areas. Where specific events are concerned, there, too, there must be surprises. The fact is that even a rich nation confronts the necessity of choice.

In summary then, let me review the five points that I'd like to emphasize through this brief presentation. First, one can measure the strength of science either in terms of its efforts or in terms of its results. These two approaches present rather different sorts of problems and have to be addressed by rather different sorts of means.

Second, as science progresses, the situation of diminishing returns prevails. Greatly increased efforts are needed to achieve the same volume of further really significant results. In advancing the frontiers of science, we're caught up in an inexorable process of cost escalation.

Third, we cannot hope to predict just where the scientific discoveries of the future will come and just what they will be. As regards matters of detail, the future of that basic science lies behind an impenetrable veil.

Fourth, if we hope to foster science effectively, we must do so in terms of broad areas of research rather than narrowly targeted particular problems.

And fifth, last, not even a rich superpower can expect to provide the leading edge at the innovative forefront of every branch of scientific research. On the other hand, by a suitably focused targeting of investment, one could reasonably hope to determine those general areas of science in which to maintain a lead over the competitors.

This concludes my remarks. Thank you very much for your patient attention.

[The prepared statement Dr. Rescher follows:]

OBSERVATIONS ON MEASURING THE STRENGTH OF SCIENCE AND SCIENTIFIC PROGRESS

Nicholas Rescher

Director
Center for Philosophy of Science
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1. Measuring the Strength of Science[1]

The strength of science can be measured either in terms of the _input_ of resources into scientific work, or in terms of the _output_ of results from it. Rather distinct things are at issue.

Input is a matter of viewing science as akin to an industrial enterprise. From this standpoint, one considers the number of scientific workers, the number and size of scientific facilities (laboratories, observatories, institutes, research stations, etc.), the scale and sophistication of their equipment, and the like. In the final analysis, this sort of thing can be measured in budgetary terms -- the investment of money.

The output of scientific work must be assessed in terms of its _results_. It represents the products of scientific efforts by way of the publications, procedures, and technological devices that are the products of work in this domain. This output itself can be assessed in two ways, either

1) _quantitatively_ in terms of the _number_ of experiments, observations, calculations, or findings. (In large measure this sort of thing can be tracked by counting scientific publications).

2) qualitatively in terms of the significance of findings. What matters here is the big discoveries, the important breakthroughs in understanding or in technological mastery over nature. Here we deal not with the quantity of results, but with their importance.

The quality of scientific output is something that is much harder to measure than its sheer quantity. Difficult, but not impossible.

The contributions to science made by a group of scientists (be it those in a single institute or in an entire nation) can be assessed both by objective and by subjective criteria.

Subjective criteria are a matter of how the results achieved by scientists are perceived in the beholding eyes of their colleagues. Peer-group recognition is the key here, with Nobel prizes or honorary degrees or invitations to major international conferences all serving as indicators. The academic marketplace is also a useful source here: the ways in which students flock to teachers and to problem areas tends to reflect "where the action is." Beyond question, citation indexing is the most sophisticated of these subjective criteria. (However this is a theme I shall not develop further here.)

Objective criteria, by contrast, reflect the applications and the technological implementations of science. Here the construction of more powerful instruments, the introduction of new products or methodologies, and performance-oriented data such as the speed of problem-solving in computations, or the lowering of death rates from disease are sample indicators.[2] In the final analysis, the efficacy of its applictions constitutes the best objective indicator of scientific strength and progress.

2. Input-Output and Cost-Benefit Relations: Diminishing Returns

Viewed in quantitative terms, the relationship between input into scientific work and output is roughly linear. A straightforward proportionality obtains: In general, if one increases expenditures by two, one can secure twice as many workers (facilities, publications, etc).

But all available evidence suggests that the relationship between input and output when the latter is construed qualitatively, is definitely nonlinear. Performance in science is not related to cost by a relationship of proportionality. Progress in the way scientific findings at the level of first-rate results is something that is increasingly expensive. (Consider an analogy: to buy a vehicle that performs twice as well as the average car, one has to spend a considerably more than twice as much.) In science we confront a situation of diminishing returns where further improvements involve sharply increasing marginal costs.

The reason for this is not far to seek. Progress in natural science depends critically on the availability of an increasingly powerful and sophisticated technology of observation and experimentation. The potential of any given level of performance is soon exhausted and one must move on to an increasingly effective -- and expensive -- new state of the technological art. In science as in war we cannot win the battles of the present with the armaments of the past. We are involved in a technological arms race against nature where the demands of enhanced performance commit us to a technology of investigation that is increasingly sophisticated -- and expensive.

Just what sorts of returns can we expect to get for increased expenditures? This question is not as simple as it sounds. Before dealing with it, let us back up a bit to review some fundamentals.

3. The Difficulties of Scientific Prediction

Scientific progress is unpredicatable at the level of detail. We cannot tell in advance what the specific answers to our scientific questions will be. It would after all, be quite unreasonable to expect detailed prognostications about the particular content of scientific discoveries. If we could predict discoveries in detail in advance of the fact, then we could make them in advance.[3] It may be possible in some cases to speculate that science will solve a certain problem, but how it will do so lies beyond the ken of those who antedate the discovery itself. In matters of scientific importance, then, we must be prepared for surprises -- and this is true not only in basic science, but to some extent in applied science as well. Commenting shortly after the publication of Frederick Soddy's speculations about atomic bombs in his 1930 book Science and Life, Robert A. Millikan, a Nobel laureate in physics, wrote that "the new evidence born of further scientific study is to the effect that it is highly improbable that there is any appreciable amount of available subatomic energy to tap."[4] In science forecasting, the record of even the most qualified practitioners is poor.

The substance of future science inevitably lies beyond our present grasp. After Pasteur had shown that bacteria could come only from pre-existing bacteria, Darwin wrote that "it is mere rubbish thinking of the origin of life; one might as well think of the origin of matter."[5] One might indeed! The scientific future is inaccessible to even the ablest of present-day workers. And, given that it is effectively impossible to predict the details of what future science will accomplish, it is not less impossible to predict in detail what future science will not accomplish.

4. Furthering Scientific Progress

The circumstance that one cannot predict particular scientific innovations and findings means that we just do not know how to predict scientific discoveries at the level of detail. We are often in a position to know that further scientific effort well yield payoffs in a certain broad area, but we cannot in advance of the fact pinpoint just *where* -- and are frequently surprised. Scientific exploration, like geographic exploration, is characterized by unlooked-for findings. By funding Columbus, Ferdinand and Isabella did not get a short-cut to India, but they did launch a vast empire for Spain. Antarctic exploration has not as yet yielded hoped-for returns in ores and minerals, but has greatly contributed to seismology and long-range weather forecasting.

Science management is somewhat like airial bombardment was in World War II. It would be nice to achieve the precision of going for particular preselected targets, but the resources at our disposal just do not allow it. In natural science, it is only by working across rather broad areas that can be confident of a pay-off. Short of making the effort, we cannot tell just where those weak spots are where nature will yield further discoveries as we press her harder in scientific inquiry.

There is thus little point in being overly mission-oriented -- inseeking to identify particular problems that it would be nice to solve, and then throwing money and resources at them. The state of the art is frequently just not be ripe for the resolution of particular scientific problems. A far more promising approach is to identify fruitful areas where new methods and instrumentalities open up new opportunities. The time has to be ripe in terms of knowledge already achieved for further developments to be realized. (The Einstein proposal reached President Roosevelt at just the right time -- a decade earlier it would have been premature. That too is a lesson of the Millikan episode mentioned above.) The wise management of scientific resources calls for a strategy based on the exploitation of new opportunitites (methods, issues), rather than for one on identifying particular problems and addressing them by whatever means are currently at hand. It certainly makes sense in applied to target the specific technological implementation of existing theory. (That, presumably, is what the "Strategic Defense Initiative" is about.) But in basic science a more broad-brush approach is in order.

The need for steadiness and continuity -- of institutional stability and tradition -- in the prosecution of basic science should also be recognized in this connection. One can count on the fingers of one or two hands the institutes in which the double helix model and the cracking of the genetic code could have been developed. Where such strong foci of capatibility exist, they deserve support and encouragement -- and the necessary freedom of operation to shift effort to where the opportunitites of the moment indicate its profitable application.

5. Keeping Ahead in the Science Race

Even though the relationship between investment in scientific work and the substantive strength of science is a complicated one, there clearly is a connection here -- the more one invests in a given branch of science, the more substantive progress one can expect to make within it. The more amply one endows its research centers with manpower, equipment, and supportive resources, the more advances one can expect. What the nonlinearity of the relationship between investment and return means, however, is that in order to maintain even a modest edge over the science of a rival center or nation, one must make a substantially larger investment. To stay ahead in basic science is not just somewhat expensive, but very expensive.

One consequence of this is that not even a very affluent nation can expect to keep its science ahead of the competition all across the board. The more it tries for breadth and universality, the more it must expect that a competitor that targets its resources more narrowly in particular areas will be able to outstrip it in those limited chosen fields. Scientific competition is like international sporting competition in this -- think not even a the great powers can expect to win every Olympic event. One cannot expect to be first all across the board. The difference is that in science, unlike sport, one is in a better position to select those particular areas in which one is going to win. (But -- to repeat -- this is indeed a matter of areas -- with specific events there can be surprises.) The fact is that even a rich nation confronts the necessity of choice.

7. Conclusion

In summary, let me review the five points that I would like to emphasize on the basis of this brief presentation:

1. One can measure the strength of science either in terms of its efforts or in terms of its results. These two approaches present very different sorts of problems.
2. As science progresses, a situation of diminishing returns prevails -- greatly increased efforts are needed to achieve the same volume of further really significant results. In advancing the frontiers of science we are caught up in an inexorable process of cost-escalation.
3. We cannot hope to predict just where the scientific discoveries of the future will come and what they will be. As regards matters of detail, the future of basic science lies behind an impenetrable veil.
4. Accordingly, if we hope to foster science effectively, we must do so in terms of broad areas of research rather than narrowly targeted particular problems.

5. Not even a rich superpower can expect to provide the leading edge at the innovation forefront of every branch of scientific research. On the other hand, by a suitably focussed targeting of investment one can reasonably hope to determine those general areas of science in which to maintain a lead over our competitors.

Notes

1. Statement prepared as testimony before the Committee on Science and Technology of the U.S. House of Representatives on 16 April 1986.

2. A useful compendium of "science indicators" is published from time to time with that title under the auspices of the National Science Board of the National Science Foundation.

3. As one commentator has wisely written: "But prediction in the field of pure science is another matter. The scientist sets forth over an uncharted sea and the scribe, left behind on the dock is asked what he may find at the other side of the waters. If the scribe knew, the scientist would not have to make his voyage" (Anonymous, "The Future as Suggested by Developments of the Past Seventy-Five Years," Scientific American, vol. 124 [1920], p. 321).

4. Quoted in Daedalus, vol. 107 (1978), p. 24.

5. Quoted in Philip Handler, ed., Biology and the Future of Man (Oxford, 1970), p. 165.

References

This statement is based on data developed in two of the presenter's books, *Scientific Progress* (Oxford: Blackwells, 1978) and *The Limits of Science* (Berkeley and Los Angeles: University of California Press, 1985). Much further literature is cited in these two books.

DISCUSSION

Mr. Fuqua. Thank you very much for a very interesting discussion.

On the last point that you made, looking from the perspective of the Congress in trying to establish science policy as the funding agent, how do we target our investment to meet changing times?

For instance, in recent years, there's been a great emphasis on computer science, but how do we make sure we're targeting the right ones?

Dr. Rescher. It seems to me there are two basic principles of selection. One is fundamentally need-oriented. There may simply be certain areas in which so pressing a need exists from the angle of health or national defense that one has to support those areas of science that are relevant to it. That seems to me to be plain enough.

The other mode of targeting seems to me to be something much more difficult; namely, it's a matter of identifying, as it were, targets of opportunity. There are domains where, through innovations in technology or intellectual technology—that is to say, new methods—new ways of looking at old or existing problems are opened up and new instruments are made available—of course, computers and supercomputers are an example of that—and it's plausible to target resources in line with such opportunities to push those areas of investigation where these new methods afford the hope or possibility of significant advances.

Mr. Fuqua. In trying to establish scientific priorities, is the scientific apparatus—and I'm thinking of the things like the National Science Foundation and the various universities that are involved in research—will they be in the forefront of leading to this targeting or policy direction, or is it appropriate for the Government agencies, such as the Congress, to get involved into those areas? After all, we're not scientists. I think what I'm trying to say—is there a gap or a void?

Dr. Rescher. Yes, yes.

I don't think that the Congress can preempt the kind of judgment that the National Science Foundation, or the National Academy of Sciences, is designed to provide.

The thing, I think, that the Congress is uniquely in a position to provide is a certain degree of countering to a very natural human tendency once any endeavor, even as revolutionary and innovative an endeavor as scientific inquiry—once any endeavor is institutionalized. There are established interests, established networks, established ways of doing business, and what it seems to me the Congress can provide is an occasional shaking up, an occasional bit of noise or nuisance injected from without that forces people to rethink fundamentals.

Let me give you the only good analogy that comes to mind, and it's the relation of Winston Churchill to the military establishment in World War II. He did not take the war out of the hands of the generals and run it for them; he provided them with a variety of prods and pushes to make sure that the job which, as it were, they alone were capable of doing was done not in a routine and—how shall I say—simply "business as usual" sort of way, but that there was an external impetus to making sure that what was done was done thoughtfully, and in any bureaucratic venture there is that tendency for thought to be replaced by routine. I think the Congress can make sure that issues are rethought from time to time.

Mr. **Fuqua**. Then, in getting public support for basic research, you very eloquently described it as something that is very difficult to define, paraphrasing you, that many times people wish to know what benefit am I getting from my tax money? As a philosopher, can you help in communicating that to the populace, that you have to have faith in the scientific community?

Dr. **Rescher**. I think that's a very, very—clearly, a very difficult lesson to convey, because one is being asked to fund through public resources very sizable amounts of money on projects whose ultimate payoff is, as all scientific work is bound to be to some extent, uncertain.

Mr. **Fuqua**. And may be far down the road.

Dr. **Rescher**. Yes—and may be very long term.

I don't think that either scientists or philosophers are in a position to do more than help a very little bit there, and I think the place where we are in a position to help is in our discharge of the teaching mission. That is, I think we can, insofar as that part of the electorate which goes to college and institutions—high schools, institutions of learning, are concerned, we can try to instill an awareness of the fact that a modern economy and a modern society is dependent on the fruits of basic science ultimately filtered through technology, and that those are long-range concerns, and that we have to make sure that the wells are primed from time to time.

Mr. **Fuqua**. Thank you.

Mr. Brown.

Mr. **Brown**. Mr. Chairman, I apologize for missing the statement of Dr. Rescher, but I want him to be aware of the fact that I personally give a great deal of priority to this question of the philosophy of science. I'm looking forward to reading your statement with great interest.

Amongst the many problems we have here with the support for science is the whole question of the philosophical basis for that support, and we need to clarify that issue much more than we have in the past.

Just off the top of my head, I note that you're discussing input and output relationships. Have you discussed, or can you comment on, the aspect of support for science that involves merely the question of utilizing to the fullest the scientific interests of our best minds in this country? In other words, providing opportunities in scientific exploration and research for those who are highly motivated and highly qualified. To what degree should this be a factor in our support for the scientific establishment, aside from input and output, as a matter of providing human satisfaction?

Dr. **Rescher**. Yes. The matter of human satisfaction is very difficult, I think, to address and may not be, in the final analysis, the single most important factor in the picture.

The thing about science is that for someone minded with those kinds of interests, it is intensely interesting, and I think that there are any number of people out there among my scientific colleagues who would pursue scientific work not only in the presence of encouragements but even in the presence of discouragements, because they themselves have been caught up in the internal fascinations of the field and the intellectual rewards of working in it.

The difficulties seem to me to be two. One is that for that kind of an attitude to develop, someone has to be enlisted intellectually into the scientific domain in the first place, and that means that the business of teaching, and especially teaching at the level of the upper grades of grade school and high school, is crucially important, because unless people are brought into a scientific interest at that stage, they will not stay there, and if they are discouraged from it at that stage, as they all too often are, they will never drift back into it later on.

That seems to me to be the important crucial point of the significance of supporting not scientific work but the recruitment of scientists, the quality of training in the precollegiate years, and especially the quality of conveying the message of the value and interest and enthusiasm that's connected scientific work, and of course, as we know, we have very, very great failings in that regard. Many young people are turned off from mathematics and science at a time of life when they should be set afire by it.

Mr. **Brown**. Well, I'm glad to hear you make that point, because I think it's one that the committee has given a great deal of thought to, also.

May I ask you specifically about the field of philosophy? You don't get much Federal support, do you?

Dr. **Rescher**. Philosophy is, certainly even more than science, one of those fields where people become involved in the absence of positive encouragement through incentives, external incentives as apart from the inner fascination of the field. I think it's a classical example of that.

Mr. **Brown**. So philosophy flourishes on the basis of its support by not the Federal Government but by State funding and private support for institutions of higher learning.

Dr. **Rescher**. Yes. But it of course flourishes, insofar as it does, on a very, very modest statistical basis. If we had as many mathematicians or physicists or chemists as we have philosophers, we would be in very, very sad straits.

Mr. **Brown**. You're fortunate that you don't require the same kind of expensive facilities.

Dr **Rescher**. It's a big help to be able to work with paper and pencil.

Mr. **Brown**. Yes.

Thank you, Mr. Chairman.

Mr. **Fuqua**. Thank you very much for joining us this morning.

Mr. **Fuqua**. Our next witness will be Dr. Eugene Garfield, the president of the Institute of Scientific Information, from Philadelphia.

Dr. Garfield, we're very pleased to have you this morning, and you may proceed.

[A biographical sketch of Dr. Garfield follows:]

Eugene Garfield, Ph.D.

Dr. Garfield is the founder of the Institute for Scientific Information. ISI produces a broad spectrum of publications, services, and systems designed to monitor and condense the flood of published scientific and technical information. Over 600 people are employed at ISI, which is widely recognized as a pioneer in the information-systems field in the United States and abroad.

Prior to founding ISI, Dr. Garfield held various positions, as a chemist and an information scientist. In 1951, he joined Johns Hopkins University's Welch Project for evaluating methods of automated indexing, where he developed the concepts that were to form the basis for ISI products. In 1956, he founded an information engineering consulting firm called Eugene Garfield Associates. In 1960 its name was changed to the Institute for Scientific Information. ISI remains a private, non-governmental organization.

Dr. Garfield's education includes a B.S. (1949) in Chemistry and an M.S. (1954) in Library Science from Columbia University where he was the first Grolier Society Fellow (1953-54). He later received received a Ph.D. (1961) in Structural Linguistics from the University of Pennsylvania. His doctoral dissertation applied modern linguistics to the problem of automatic indexing of chemical information.

A member of the American Society for Information Science since it was founded, Dr. Garfield won the ASIS Award of Merit in 1975. He is also a Fellow of the Institute of Information Scientists of London.

He is a 30-year member of the American Chemical Society. He received the Herman Skolnik Award from the Division of Chemical Information Science in 1977 and the Patterson-Crane Award for chemical information in 1983. Dr. Garfield is also a member of the Chemical Notation Association and received the CNA Award in 1980.

Dr. Garfield helped establish the Information Industry Association (IIA) in 1968 and has served as President and Chairman of its Board of Directors. In 1977, he received the IIA's Hall of Fame Award.

In 1984, he became the first information scientist to receive the John Price Wetherill Medal, awarded by The Franklin Institute in Philadelphia. In 1984 he also received the first Derek Price Memorial Medal in scientometrics.

Dr. Garfield is also a member of the Institute of Electrical and Electronic Engineers, Medical Library Association, Special Libraries Association, and Association for Computing Machinery. He is a member of the Authors League of America, the National Association of Science Writers, and the Society for Scholarly Publishing. He is an elected Fellow of the American Association for the Advancement of Science and has served as Chairman of Section T (Information, Computing, and Communication).

In 1978, he became a member of the Rockefeller University Council Dr. Garfield is also a member of the Board of Directors of *Annual Reviews*. He serves on the editorial boards of *Scientometerics* and the *Journal of Information Science*. He also serves on the advisory committee for the John Scott Award for useful inventions, presented annually by the City of Philadelphia.

Dr. Garfield has served on several committees of the National Academy of Sciences including those on Cardiovascular Literature. Oceanographic Documentation, and Scientific and Technical Communications (SATCOM). He also participated in the White House Conference on International Cooperation and has testified before several committees of Congress on matters relating to information and copyright policies of the United States Government. He was a member of the National Commission on Libraries and Information Science Task Force on a National Periodicals System.

Dr. Garfield is the author of numerous papers on information science. He has lectured widely in the United States and throughout the world. He delivered the 1983 Ranganathan Memorial Lectures, sponsored by the Sarada Ranganathan Endowment for Library Science in Bangalore, India. In 1984 he spoke to the Science, Technology, and Society Colloquium and also to the Three Cultures Dialog at The Pennsylvania State University. In 1985, he delivered the Alza Lecture of the Biomedical Engineering Society and the Miles Conrad Memorial Lecture of the National Federation of Abstracting and Indexing Services and was a guest lecturer at the Jackson Laboratory, Johns Hopkins University, and the Canadian Association for Information Science.

At the University of Pennsylvania, he is Adjunct Professor in the Department of Computer and Information Science. He holds U.S. and foreign patents on devices for selective copying and reproduction of information.

For the past several years he has written a weekly column called *Current Comments*. Over 800 of these essays and other works are now collected in seven volumes of *Essays of an Information Scientist*, which won the American Society for Information Science award for best information science book of 1977. His book *Citation Indexing--Its Theory and Application Science, Technology and Humanities* was published in 1979 by Wiley and reprinted in 1983 by ISI Press.

STATEMENT OF DR. EUGENE GARFIELD, PRESIDENT, INSTITUTE FOR SCIENTIFIC INFORMATION, PHILADELPHIA, PA

Dr. GARFIELD. Thank you very much.

I'm going to read my prepared statement quickly, and I'll be glad to answer your questions.

There are a number of appendices—attachments—which I've prepared or brought with me which, of course, are available, some of which are not on the table here, which I will leave with the staff people, which you may find of interest. Actually, there's an awful lot of information already been published on this subject.

Very briefly, the Institute for Scientific Information, of which I'm the president, is a private company organized almost 30 years ago to provide scientific information to industrial and academic researchers.

Something was said a little while ago that reminded me of a time I came here about 25 years ago to testify before Senator Humphrey and, I believe, Claude Pepper, among others, about what sounds like the same problem to me today in response to Congressman Brown's question, and that was why ISI was started.

It's best known to the scientific community as the publisher of *Current Contents*—there's a copy here—which has appeared weekly for the past 29 years. We also publish the *Science Citation Index*, and this bimonthly publication is cumulated annually and then every 5 years. I had thought to be dramatic and bring with me a copy of 1 year's volumes, but if you went over to the library, the annual output is 15 volumes, the size of the *Britannica.*

The SCI now covers over 30 years of the literature, and we've indexed over 10 million articles to date. We also produce a number of other services that I won't discuss here today but including the *Social Sciences Citation Index* and the *Arts and Humanities Citation Index.*

Since many Members of Congress are trained as attorneys, SCI can be described simply as the *Shepards Citations of Science.* Indeed, our *Social Sciences Citation Index* covers over 3,000 journals, including 200 law reviews.

Were there adequate time, I would explain how these compilations facilitate the retrieval of scientific information, and I would also explain how Scisearch and Social Scisearch, the electronic versions of these data bases, are used throughout the world.

However, my primary purpose today is to explain how these databases are used for the study and formation of science policy. Indeed, together with colleagues like the late Derek deSolla Price, among others, we have pioneered the field called "Scientometrics." Incidentally, the biannual reports of the National Science Board called "*Science Indicators*" include many indicators that are derived from the *Science Citation Index.* These generalized indicators do not, however, provide detailed intelligence about specific research areas where the United States may be lagging or pioneering, sometimes unknown to policymaking bodies who are, like most of us, unable to cope with the information flood.

Some of the more elementary uses of the SCI involve straight publication counts, as you've heard from the previous speaker. In these studies, publication output for each country is measured as an indicator of relative strength. A more sophisticated measure, the relative impact of these publications, is measured by determining how often they are cited. These data can be sorted by country, language, subject, and a number of other parameters, and through these unobtrusive studies some rather remarkable insights on international and national research effort can be obtained.

As an extension of these data, we have also pioneered a technique called "science mapping", and the maps which I have appended show you some of those examples. The information is limited to 1983 and 1984; the data for 1985 will be available within a few weeks. This type of information can also be examined on personal computers. The analyst can obtain a detailed view of any research area, and he can focus in from a broad area like molecular biology down through various generic levels to a specific "research front" or topic. Each front is associated with a group of individuals, institutions, or their publications—the so-called invisible colleges—and in the examples we have zeroed in on, for example, the Beta Thalassemia gene in one case and human interferon gene cloning in the other.

In yet another appendix, year-to-year changes can be observed in what we call historiographs. The example provided is in monoclonal antibodies.

One can use these data to observe the contribution of each country to small or larger research fronts. For example, one can find out who is producing the highly cited publications in an area, and who is publishing and citing in this area. This way, we can identify the specific areas in which a country is pioneering and those in which it may be following or lagging behind.

There are almost an infinite number of ways these data can be used for science policy. As one example, I've also appended a reprint from a journal called *Science and Public Policy* showing an analysis of Third World research.

There are some important caveats in the potential use of these kinds of statistical data. All such data should be used carefully by understanding the limitations. Like all statistical data, they can be used and misused. The information can aid policymakers by indicating where the action is and even possibly where it seems to be going, certainly where it has been, but the data should only be used to make better informed decisions about which areas should or should not be funded. Whether to accelerate funding in rapidly emerging fields of research or not, is dependent upon many other subjective factors.

Scientists and science research deals in ideas and concepts. It's difficult to measure productivity and originality of ideas directly. However, the basis of scientific progress is the publication of scientific papers, and the paper can be taken as an accurate measure for analyzing the research activity, creativity, and impact of individuals and countries.

I'd like to turn to a discussion of the Nobel Prizes. This has become the universal symbol of excellence in science. But the public is unaware that Nobelists represent a tiny fraction of the individuals that I have described in a number of publications as being of Nobel class.

Over 25 years ago, we studied data for Nobel Prize winners, and found that there is a publication pattern which is repeated in over 90 percent of the winners. Nobel Prize winners published 10 times the number of papers, and were cited at least 30 to 50 times as often as the average scientist. Put another way, all Nobel Prize winners fall into the top one-half percent of cited authors, and 90 percent of them are found among the top one-tenth of 1 percent. A Nobelist is literally 1 in 1,000!

There are over 1 million publishing scientists in the world, about 325,000 in the United States of America. If one-half of 1 percent of these are of Nobel class, then about 5,000 scientists in the world, and 1,600 in the United States of America are involved in this estimate. Obviously, the small number of prizes each year is too small a sample of Nobel-class scientists to have statistical validity for science policy analysis, and do not accurately reflect national strengths in science. By using the comprehensive files of ISI indexes, you can study national strengths by viewing a much larger portion of the scientific landscape and in greater detail.

How do you go about determining which of the world's 1 million scientists are of Nobel class? Now there's a kind of electoral process that goes on in science in which scientists cast their votes. These are the citations or references that appear in the papers they publish. Statistical analyses show that in spite of occasional deviant citation behavior, citation distributions follow what is commonly called the 80/20 rule. A small percentage of journals or authors produce a large percentage of the papers, and, similarly, an even smaller percentage of authors or institutions or countries receive a large percentage of the citations. For example, I have appended a 10-part essay study from *Current Contents* which includes a list of 1,000 citation classics. These papers received 100 times as many citations as the average paper.

I do not have time to explain how we can standardize our data so as to take into account the varying sizes of fields like mathematics and medicine, or even philosophy, but this is certainly relevant and possible. When using our data, we adjust thresholds of publication productivity and/or citation frequency according to the field involved. However, there is no doubt in my mind that a study of the nation origin of the papers written by the authors of Nobel class would give reliable indicators of relative strengths and weaknesses.

It is not generally known that there has never been a complete census of scientific publications. None of the traditional abstracting services is absolutely comprehensive. The National Science Foundation sponsored studies many years ago which demonstrated that there were major gaps in coverage by a combination of discipline-oriented services—this was particularly true at the time of Sputnik—and coverage must be multidisciplinary or gaps occur in journals like *Nature*, and *Science*, and hundreds of others, and many dynamic areas, especially today, involve many different disciplines, and that is why in the Institute for Scientific Information we implemented the unprecedented policy of absolutely complete coverage of the world's most important journals, and while we cover the cream of Soviet, Japanese, and Third World literature, there are many more one would include for a complete census. Thus, important trends and currents in certain research activity of these major scientific communities may be underrepresented, and our data base would need to be enlarged both currently and retrospectively to facilitate indepth chronologic studies over long periods. In that way, we can observe the growth and decline of science productivities in various fields.

So, in conclusion, there can be no doubt that our comprehensive citator files can be used to aid in science policy decisions. Our data are already used regularly by several United States and foreign government agencies. We are now seeking to establish a prominent international science indicators data base. A small group of United States and foreign government agencies will be asked to support and monitor this system. Each country would then have access to a computerized data base that would permit science policymakers and analysts to conduct econometric-like studies in response to questions from groups like your own.

ISI's existing coverage of about 1 million papers per year could be expanded as deemed necessary to take in more Third World or other foreign literature, and I'd also like to stress that you have to make a very careful distinction between science and technology literature, and a lot of the questions asked before relate to the differences in the two.

Not coincidentally with the above, we are launching, in August, *The Scientist,* a newspaper for professional researchers. We expect to publish, regularly, news and opinions on science policy data of wide interest to the scientific community and to the public. This will take various forms, including an extensive op-ed section, statistical reports, survey reviews such as provided in the *ISI Atlas of Science,* and a wide variety of editorial features based on *Citation Classics,* et cetera. We expect this newspaper to be an authoritative source for science policy information of interest to the scientific community and all those concerned with its funding and impact on society.

All these activities are consistent with ISI's mission to remain in the future, as in the past, at the forefront of science policy analysis.

Thank you.

[The prepared statement Dr. Garfield follows:]

April 11, 1986

THE RELATIONSHIP BETWEEN INTERNATIONAL SCIENCE INDICATORS, NOBEL CLASS SCIENCE, AND SCIENCE MAPPING IN THE FORMATION OF SCIENCE POLICY

STATEMENT BEFORE HOUSE OF REPRESENTATIVES COMMITTEE
ON SCIENCE & TECHNOLOGY
APRIL 16, 1986

by

EUGENE GARFIELD, Ph.D., PRESIDENT,
INSTITUTE FOR SCIENTIFIC INFORMATION
PHILADELPHIA, PA 19104

The Institute for Scientific Information is a private company organized almost thirty years ago to provide scientific information to industrial and academic researchers. ISI is best known to the scientific community as the publisher of _Current Contents_, which has appeared weekly for the past 29 years. We also publish the _Science Citation Index_ (_SCI_). This bi-monthly publication is cumulated annually and then every five years. _SCI_ now covers over thirty years of the literature. We've indexed over 10 million articles. We also produce a number of other services, including _Current Abstracts of Chemistry_ and _Current Chemical Reactions_, _Social Sciences Citation Index_ and the _Arts & Humanities Citation Index_.

Since many of you are trained as attorneys, SCI can be described simply as the Shepards Citations of science. Indeed, our Social Sciences Citation Index (SSCI) covers over 3,000 journals, including 200 law reviews.

Were there adequate time, I could explain how these compilations facilitate the retrieval and dissemination of scientific information. I would also explain how SCISEARCH and SOCIAL SCISEARCH, the electronic versions of these databases, are used throughout the world. However, my primary purpose today is to explain how these databases are used for the study and formation of science policy. Indeed, together with colleagues like the late Professor Derek deSolla Price, among others, we have pioneered the field called "Scientometrics." Incidentally, the bi-annual reports of the National Science Board called "Science Indicators," include many indicators that are derived from the Science Citation Index. These generalized indicators do not, however, provide detailed intelligence about specific research areas where the U.S. may be lagging or pioneering, some times unbeknownst to policy making bodies who are unable to cope with the information flood.

Productivity vs Impact

Some of the more elementary uses of SCI involve straight publication counts. In these studies, publication output for each country is measured as an indicator of relative strength. A more sophisticated measure, the relative impact of these publications, is measured by determining how often they are cited. These data can be sorted by country, language, subject, etc. Through these unobtrusive studies, some rather remarkable insights on international and national research effort can be obtained. As an extension of these data, ISI has also pioneered a technique called "science mapping." The appended maps cover 1983 and 1984. The information for 1985 will be available within a few weeks. This type of information can also be examined on personal computers. The analyst can obtain a detailed view of any research area. He can focus in from a broad area like molecular biology down through various generic levels to a specific "research front." Each front is associated with a group of individuals, institutions and their publications--- the so called invisible colleges. In the examples, we've zeroed in on the Beta Thalassemia gene in one case and human interferon gene cloning in the other.

In yet another appendix, year-to-year changes can be observed in what we call "historiographs." (Monoclonal antibodies).

One can use these data to observe the contribution of each country to small or larger research fronts. For example, one can find out who is producing the highly cited publications in an area and who is publishing and citing in this area. This way we can identify the specific areas in which a country is pioneering and those in which it may be following or lagging behind.

There are almost infinite ways these data can be used for science policy. As one example, I have appended a reprint from Science and Public Policy, showing an analysis of Third World research.

There are some important caveats in the potential use of these kinds of statistical data. All such data should be used carefully by understanding the limitations. Like all statistical data, they can be used and misused. The information can aid policy makers by indicating where the action is and even possibly where it seems to be going---certainly, where it has been. But the data should only be used to make better informed decisions about which areas should or should not be funded. Whether to accelerate funding in rapidly emerging fields of research or not, is dependent upon many other subjective factors.

Scientists and science research deals in ideas and concepts. It is difficult to measure productivity and originality of ideas directly. However, the basis of scientific progress is the publication of scientific papers, and the scientific paper can be taken as an accurate measure for analyzing the research activity, creativity and impact of individuals and countries.

Nobel Prizes vs Of Nobel Class

The Nobel Prize has become the universal symbol of excellence in science. But the public is unaware that Nobelists represents a tiny fraction of the individuals I have described as being of Nobel Class.

Over 25 years ago, ISI studied data for Nobel Prize winners and found that there is a publication pattern which is repeated in over 90% of the winners. Nobel winners published ten times the number of papers and were cited at least 50 times as often as the average scientist. Put another way, all Nobel Prize winners fall into the top one half percent of cited authors---90% are found among the top one-tenth percent. A Nobelist is literally one in a thousand!

There are over 1,000,000 publishing scientists in the world; about 325,000 in the USA. If 0.5% of these are of Nobel Class, then about 5,000 scientists in the world and 1,600 in the USA are involved. Obviously, the small number of Nobel prizes each year is too small a sample of Nobel Class scientists, to have statistical validity for science policy analysis and do not accurately reflect national strengths in science. By using the comprehensive files of ISI indexes, you can study national strengths by viewing a much larger portion of the scientific landscape and in greater detail.

How do you go about determining which of the world's 1,000,000 scientists are _of Nobel Class?_ There is a kind of electoral process that goes on in science in which scientists cast their votes. These are the citations or references that appear in the papers they publish. Statistical analyses show that in spite of occasional deviant citation behavior, citation distributions follow, what is commonly called, the 80/20 rule. A small percentage of journals or authors produce a large percentage of the papers. Similarly, an even smaller percentage of authors or institutions or countries receive a large percentage of the citations. For example, I have appended a ten-part essay from _Current Contents_ which includes a list of 1,000 _Citation Classics_. These papers received 100 times as many citations as the average paper. ("1,000 Most-Cited Articles" - 10 part essay.)

I do not have the time to explain how we can standardize our data so as to take into account the varying sizes of fields like mathematics and medicine. But this is certainly relevant and possible. When using our data, we adjust thresholds of publication productivity and / or citation frequency according to the field involved.

However, there is no doubt in my mind that a study of the national origin of the papers written by the authors _of Nobel Class_ would give reliable indicators of relative strengths and weaknesses.

It is not generally known that there has never been a complete census of scientific publications. None of the traditional abstracting services is absolutely comprehensive. The National Science Foundation sponsored studies which demonstrated that there were major gaps in coverage by a combination of discipline oriented services. Coverage must be multi-disciplinary or gaps occur in covering Nature, Science, and hundreds of other journals. Many dynamic areas involve many disciplines. That is why ISI implemented the unprecedented policy of absolutely complete coverage of the world's most important journals. While we cover the cream of Soviet, Japanese and Third World literature, there are many more one would include for a complete census. Thus, important trends and currents in certain research activity of these major scientific communities may be underrepresented. Our database needs to be enlarged both currently and retrospectively to facilitate indepth chronologic studies over long periods. In that way, we can observe the growth and decline of science productivities in various fields.

In conclusion---there can be no doubt that our comprehensive citator files can be used to aid in science policy decisions. Our data are already used regularly by several U.S. and foreign government agencies. We are now seeking to establish a permanent International Science Indicators Database. A small group of U.S.

and foreign government agencies will be asked to support and monitor this system. Each country would then have access to a computerized database that would permit science policy makers to conduct econometric-like studies in response to questions from groups like your own. ISI's existing coverage of 1,000,000 papers per year could be expanded, as deemed necessary, to take in more Third World or other foreign literature.

Not coincidentally, we are launching, in August, The Scientist--a newspaper for professional researchers. We expect to publish, regularly, news and opinions on science policy data of wide interest to the scientific community and to the public. This will take various forms including an extensive op-ed section, statistical reports, survey reviews such as provided in the ISI Atlas of Science and a wide variety of editorial features based on Citation Classics, most-cited authors, etc. We expect this newspaper to be an authoritative source for science policy information of interest to the scientific community and all those concerned with its funding and impact on society.

All of these activities are consistent with ISI's mission to remain, in the future, as in the past, at the forefront of science policy analysis.

EG/mm

LIST OF APPENDED ITEMS

1. Biography - Eugene Garfield, Ph.D.

2. Maps of Science for 1983 and 1984. Global and other miscellaneous maps (Ref : Page 3)

3. Monoclonal Antibodies - Historiograph (Ref : Page 3)

4. E. Garfield, "Mapping Science in the Third World," Science & Public Policy, June 1983.

5. E. Garfield, "1,000 Most-cited Articles," Parts 1-10. (Will be brought in by Dr. Garfield)

6. E. Garfield, "Citation Classics - Four Years of the Human Side of Science," #22, Current Contents, June 1, 1986.

7. "Nobelists in the 300 Most-Cited Authors, 1961-76."

8. "Nobel Prizewinners on 1,000 most-cited author list."

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DISCUSSION

Mr. **Fuqua**. Thank you very much, Dr. Garfield.

Based on some of the studies that you performed and published in recent years, using your ISI data base, have you been able to detect any change in the U.S. leadership position in science generally, or any particular field or subfields of science?

Dr. **Garfield**. I think it's generally recognized that in terms of publication output, our share of the world total is going down in various fields. I make the distinction between that and what might be the seemingly increasing share of important, well-cited, high-impact publications, but nevertheless——

Mr. **Fuqua**. How about particular fields, though, or subfields?

Dr. **Garfield**. Well, our strengths in various subfields definitely, I think, would be shown to be going down and others where we are maintaining leadership. I believe even the NSF *Science Indicators* report referred to a number of those areas, but I can't give you today specific answers on any particular fields. I'd be glad to try and find out. But I certainly think that each country can be shown to have particular strengths, whether it's in India where they excel in electrochemistry or the Russians in various areas. We certainly have no monopoly on excellence.

Mr. **Fuqua**. But has there been any significant shift in U.S. leadership or lack of leadership?

Dr. **Garfield**. Well, again, I think that when I say that we are not necessarily the leader in all of them, I think that it means that perhaps in some cases there was a time when we were, and I think you'd have to look at the quality of the output. I mean, I could say that the Russians are accelerating their publications in certain fields like biology of membranes, but that doesn't mean we've lost our leadership, but what will happen in 2 or 3 years I don't know.

Mr. **Fuqua**. We've frequently been told and through experience that the length of time for scientific research to have an impact varies greatly. Sometimes it's 6 months, sometimes it's 50 years, or maybe longer than that. In your data bases, can you tell anything about the impact time from research to——

Dr. **Garfield**. Timewise?

Mr. **Fuqua**. Yes.

Dr. **Garfield**. Absolutely. There's a series of studies which we've done which, for example, show that for a basic discovery in chemistry, that it's going to take anywhere from 1, 2, 3, 4 years more than the average important discovery in, let's say, the life sciences for the impact to be felt, and so you can plot the indicators of that impact—citations—over a long period of time, and you see that the curves are very different for certain fields of chemistry than they are for other branches of science.

Today, you would find that in some areas of age research things are moving so quickly you might see the impact indicators within 6 months, and we publish an annual series of the 100 most cited papers, and some of those papers are cited as many as 150 times within a 1-year period, which shows how quickly that basic information or that particular methodology is being absorbed.

On the other hand, there are fields where you have to train students in the particular new methodology; it may have been a great innovation reported in *Science* or some leading journal, but you're not going to see too many papers referring to that until the new generation has been trained in the use and has, in fact, absorbed the ideas. So there is that lack, and it will vary. In mathematics, it could be 10 years, 15 years in some. In the case of computer-aided tomography, you have a basic mathematical discovery that was reported 15 years before the subsequent development of the application.

Mr. **Fuqua**. You mentioned that there was a world-class or Nobel-class of scientists, about 5,000, I think. Do you have any information about the distribution of the Nobel Prize winners among the 5,000 as it relates to the various countries? Is there proportional representation or some countries have more than others?

Dr. **Garfield**. No; I think that those who are Nobel Prize winners, those numbers are pretty generally known, and the United States has dominated the Nobel Prizes for recent decades.

When you talk about scientists of Nobel class——

Mr. **Fuqua**. But is that in proportion to the Nobel-class that you referred to?

Dr. **Garfield**. I think that you would find, oddly enough, the percentage of U.S. scientists is about 70 percent there. I don't know whether that precise number is what you'd find among the Nobel Prize winners. I think that may be somewhat coincidental. And, incidentally, I didn't really mean to say that there are only 5,000; I said that were you to assume that you limited yourself to that small percentage. I believe the number is much larger; I think that there are easily 15,000 people in the world who are capable of Nobel-class work long term, but I wanted to cut that down to a very small number.

I think that for the U.S. contribution, since it is so large, the numbers may be a little bit more reliable than you would have for other individual countries where you've got—if there is one Nobel Prize winner from Hungary, let's say, or some other country like that, it's not going to necessarily represent that country's output.

Mr. **Fuqua**. Yes.

Mr. Brown.

Mr. **Brown**. Dr. Garfield, I'm tremendously impressed by the scope of your work. Very honestly, I was not aware of how well you've done. It represents a great contribution.

I think you know that this committee has been interested in the improvement of our handling of scientific and technical information for many, many years, and we've included, for example, in the OSTP Act this responsibility to do a more effective job as far as the Federal Government is concerned to put that responsibility in the Office of the President's Science Advisor. We did that 10 years ago. At that time, you weren't a very—or you weren't as influential in this field as you are today, I presume.

Dr. **Garfield**. I guess not.

Mr. **Brown**. You've made a lot of progress.

What I am interested in exploring with you is the degree to which what you have done fulfills the mandate that we were trying to put on the Office of Science and Technology policy to bring some degree of coordination and rationality to this field. How does your work relate to, for example, the work that the NSF does on the *Science Indicators?*

We provided for the development of these 5-year *Outlook in Science* publications which were intended to review the status of scientific development in promising fields. Does your work contribute to the ability to achieve what we were trying to achieve with this kind of legislative effort? Help me a little bit with that.

Dr. GARFIELD. I mentioned before that the *Science Indicators* report that comes out every 2 years, certain of the tables that appear in there are derived from the *Science Citation Index* which we publish. However, we did not put those tables together. That work is done on contracts with other people. So our data is used, but I don't think that contrary to what you said before about the change in the ISI's reputation in 10 years, we are not consulted all that much by the Government. I'm not complaining; I'm just stating a fact.

Mr. BROWN. You don't have any contracts with the Government?

Dr. GARFIELD. We have had a few research contracts, but we do not have any direct contracts with any Government agency to provide the statistical data that I mentioned earlier that we would like to put together as what I call the international statistical indicator—Science Indicators report, which would be shared by the Europeans as well as the United States agencies, because you just don't take these databases and grind them out like sausages and produce the data you want. We estimate it will take probably $1 million a year to provide the answers to all the questions that we know these kinds of agencies want answered.

Mr. BROWN. How does your work relate to that of the abstractors, like *Chemical Abstracts* and *Physics abstract*?

Dr. GARFIELD. Well, they are a friendly competitors.

Mr. BROWN. They're competitors then?

Dr. GARFIELD. Well, in the sense that they produce an abstract service for chemistry, and we also have one for chemistry, but we are a multidisciplinary organization, so we cover all the disciplines.

Mr. BROWN. You don't steal from each other, do you?

Dr. GARFIELD. No, no. It's completely separate efforts.

Mr. BROWN. Then do we have a duplication here which, in some overall sense, might be reduced in order to provide a more economical service?

Dr. GARFIELD. Well, that's a long philosophical question. I mean, it's no different than the number of different legal and financial information services that exist. You don't say because West exists there shouldn't be Lexis, or vice versa.

Mr. BROWN. Well, I say that, but the lawyers——

Dr. GARFIELD. One might say that; OK. But they are not providing precisely identical approaches. I mean, *Current Contents* is one method of putting it together, and the others are abstracts. So one is a citation index and the other digest; one is an atlas that we're trying to do. So there are a lot of different ways of approaching the information. Obviously, if all these organizations were to be centralized, you might——

Mr. BROWN. How close are we getting to the point where we can say that the field of scientific and technological information is relatively well rationalized, that there aren't huge gaps that need to be filled, that you and other competitors are meeting the needs? Frankly, this is where we would like to get to.

While I think the Government has some responsibility to recognize the need and to facilitate the filling of those needs, I don't want to see the Government involved in it if there are entrepreneurs such as yourselves who can do the job and can make a reasonable profit on it. I think that's the goal we should be looking for.

Dr. Garfield. Well, I have no doubt that we can do the job. I think the job that I referred to before that remains unfilled is satisfying the needs of special interest groups in the Government and elsewhere that want information in much greater depth.

If I say to you we cover 1 million papers a year, I say that knowing that there are, let's say, 100,000 or 200,000 Russian and Japanese papers we could add to the system, which may not change the general science indicators very much but will change knowledge about certain specific areas of research.

Mr. Brown. But you have to be concerned about a market for that. You can't do the work involved in that unless there is some market that pays for it.

Dr. Garfield. That's correct.

Mr. Brown. And you have a constant job then of market research. Do you think that if you could fill that gap you could make a profit?

Dr. Garfield. Eventually one might find that to be the case, because I think that you'd find right now less libraries and other institutions have adequate support for their libraries. The prices of these services would start to escalate to the point where they'd have to cancel them.

Now we're charging $6,000 a year for the *Science Citation Index*, which only a small number of libraries in the world—about 1,000—could afford. If we—let's say we increase our coverage by 50 percent, if it's another $2,000 or $3,000 a year, there's going to be somebody who's going to start hurting. So we have to be very careful about that sort of thing; you're absolutely right.

But if one prepares a data base for not just these purposes, not just for retrieval purposes but for indicators, you may find that that activity would support it. I mean, you're not going to make a great deal of money out of it, I'll tell you that; it's a very competitive business and takes a lot of effort to produce an additional small percentage of profit. The people who are in this business are not all in it just for profit; there's a fascination with licking this information giant, and that helps motivate us to cover more and more each year. I mean, how did we get from covering 100,000 to 1 million papers a year?

Mr. Brown. Well, fortunately or unfortunately, somewhere you've got to make a profit, because there are very few institutions that have deep enough pockets to keep operating at a loss.

Dr. Garfield. Yes. Well, I believe that if we were to watch it carefully, and if we had additional support from Government agencies and others—they don't have to duplicate what we're doing—that we could provide them the additional information they need.

Mr. Brown. Well, thank you very much, Dr. Garfield.

Mr. Fuqua. Thank you.

Mr. Slaughter.

Mr. SLAUGHTER. No questions.

Mr. FUQUA. Thank you very much, Dr. Garfield. We appreciate your being here this morning.

Mr. FUQUA. Our next witness will be Dr. Christopher Hill, senior specialist in science and technology policy at the Library of Congress.

Dr. Hill, have you had to be working in the dark lately?

Dr. HILL. Pardon me?

Mr. FUQUA. I say, have you been forced to work in the dark lately?

Dr. HILL. We've been forced to work without water.

Mr. FUQUA. Oh.

Dr. HILL. That's our main problem at the Library of Congress these days—no drinking water for the last several weeks. So we subsist on martinis from Jenkins Hill, but we're getting along.

Mr. FUQUA. Thank you for being here, and I understand you're joined by Ms. Joan Winston, a senior research assistant.

We're glad to have you this morning also.

Ms. WINSTON. Thank you.

[A biographical sketch of Dr. Hill and Ms. Winston follows:]

DR. CHRISTOPHER T. HILL

Dr. Christopher T. Hill has been Senior Specialist in Science and Technology Policy at the Congressional Research Service of the Library of Congress since September 1983. Previously, he was a Research Engineer at the Uniroyal Corporation in Wayne, New Jersey, Associate Professor of Technology and Human Affairs and of Chemical Engineering at Washington University in St. Louis, Senior Professional Staff Member at the Office of Technology Assessment, and Senior Research Associate at the MIT Center for Policy Alternatives. He has been a consultant to the National Science Foundation, the Organization for Economic Cooperation and Development, and several private firms. He is the author, co-author or editor of three books and over 50 major reports and papers in public policy and in polymer fluid mechanics. He is a registered professional engineer in the State of Missouri. Dr. Hill earned three degrees in chemical engineering: BS at the Illinois Institute of Technology in Chicago, and MS and PhD at the University of Wisconsin, Madison. Born and raised in Clarksburg, West Virginia, he lives in Bethesda, Maryland.

Joan D. Winston has been a research assistant at the Congressional Research Service of the Library of Congress since September 1984. Previously, she was an engineer at the Charles Stark Draper Laboratory in Cambridge, Massachusetts. She earned an SB in physics and an SM in Technology and Policy, both from the Massachusetts Institute of Technology. Born and raised in Gulfport, Florida, she lives in Arlington, Virginia.

STATEMENT OF DR. CHRISTOPHER HILL, SENIOR SPECIALIST IN SCIENCE AND TECHNOLOGY POLICY, CONGRESSIONAL RESEARCH SERVICE, LIBRARY OF CONGRESS, WASHINGTON, DC, ACCOMPANIED BY JOAN D. WINSTON, SENIOR RESEARCH ASSISTANT, OFFICE OF SENIOR SPECIALISTS, CONGRESSIONAL RESEARCH SERVICE, LIBRARY OF CONGRESS, WASHINGTON, DC

Dr. HILL. Mr. Chairman and members of the task force, soon after the task force was established, you asked us at the Congressional Research Service to do a study of the usefulness of the Nobel Prize awards in science as a measure of our national strength in science, and we are here today to report to you on the findings of that study.

With your permission, I'd like to highlight and summarize a few of the points in our written testimony and submit the bulk of the testimony for the record.

Mr. FUQUA. Yes, we'll make it part of the record.

Dr. HILL. Thank you.

Several attached charts on the back of the testimony support points we make in the text. They are redundant but may be useful to you as well.

The Nobel Prizes in physics, in chemistry, and in physiology or medicine are the most prestigious and well known of all the awards offered to scientists. Citizens of the United States have received an increasing share of these prizes in all three fields and in the last decade have taken 60 percent of them. The closest country to us is the United Kingdom, whose citizens have won 11 percent of the prizes in the last decade.

While the Nobel Prizes are awarded to individuals, the number of prizes won by a nation's citizens is frequently used as an indicator of that country's relative strength in science. Furthermore, the national patterns of Nobel Prize winning are often regarded as indicators of national technological capability, of national capacity for economic performance, and of national determination to improve the health of the citizenry.

Questions are raised from time to time, however, about the significance of the fact that the United States gets so many of the prizes. Some people believe that our success should be attributed as much to the fact that the United States has been the homeland of choice of many expatriot Europeans as to the quality of our indigenous research and development community, and some people have pointed out that the rank order of nations by their success in winning Nobel Prizes is not the same as the ranking of those same nations according to other measures of scientific, technological, and economic performance.

The purpose of our study, then, was to provide a more solid empirical foundation for understanding the significance of national patterns of Nobel Prize winning.

Our study for the task force was based on an empirical analysis of 229 individual winners of the Nobel Prizes in the natural sciences from 1945 through 1984. As a footnote, I might point out that 1 person won twice, and so there are actually only 228 individuals. We excluded from our study the prizes for literature and for peace as well as the more recently established prize in economics.

We compared the national patterns of Nobel Prize winning with other measures of national scientific strength, like spending on research and development, like the number of scientists working in a country, and various measures of scientific publication. The Nobel Prize performance of each country was also compared with indicators of its technological competitiveness, its economic performance, and its public health status to determine whether success in prize winning is related to these other national goals.

We wish to emphasize that the study focuses on whether Nobel Prize winning research contributes to the comparative advantage of the nation in which the research is done. We did not attempt to determine whether such research contributes to the improvement of the entire group of prize winning nations or the world as a whole.

In constructing our indicators of the Nobel Prize winning performance of each country, we made a number of assumptions, some of which are contrary to the way in which national performance in Nobel Prize winning is ordinarily reported.

First, we assumed that each prize was assigned a value of one prize unit. That is to say, we assumed that all Nobel Prize winning science is of equal importance.

Second, we assumed that when a prize was divided among two or three persons, as they often are, each winner was assigned the fraction of a prize unit specified by the Nobel Foundation.

Third, we assumed that the date of each prize was taken to be the midpoint year of the period in which the prize winning work was done and not the year in which the award was made, and that makes a very big difference, as we'll see later on.

And, finally, we assumed that each prize was assigned to the country in which the prize winning work was done, not to the country of the citizenship of the winner, which is how the prizes are awarded by the Nobel Foundation.

In order to construct the indicators, biographical data including some 30 or 35 pieces of information on each of the 229 winners since 1945 were assembled and entered into a computerized data base, which we have made available to the task force and which, by the way, we are prepared to make available to researchers in the field.

My colleague, Joan Winston, carried out this rather tedious task with enormous diligence and great good humor, but that's only one of the reasons I asked her to join me here today.

We divided the study period, 1945–84, into four decades and based our indicator of national Nobel Prize winning performance on the number of prize units accumulated in each decade by scientists who were working in each country. For some purposes, we then divided these indicators by the population in 1970 to account for country size.

Since we finished our study before the 1985 prizes were awarded, and we worked hard to be sure that we did so that we didn't have to keep updating things, the five Nobel laureates in science from 1985 are not considered in our results. We have taken a quick look at what the significance of those last five prizes might have been, and they make relatively little difference to our findings.

Let me summarize first a few of our findings then on national patterns in Nobel Prize winning.

First, whereas the citizens of 24 countries have been awarded prizes since 1945, only 14 countries have actually been what we call the host for Nobel Prize winning work since 1945. The first chart lists both those 24 and 14 countries, and you can see, as Dr. Garfield pointed out earlier, that the United States stands very high on the chart either way you look at it.

CHART 1

NATIONAL STANDINGS IN SCIENCE NOBEL PRIZE ACHIEVEMENT

1945-1984

Country	Number of Prize Winners Among Country's Citizens		Number of Prize Winners Who Did Winning Work in the Country Between 1945-1984	
	#	% of Total	#	% of Total
U.S.	114	50	85	57
U.K.	41	18	30	20
Germany	14	6	7	4.7
France	6	2.6	3	2
Japan	4	1.7	3	2
U.S.S.R.	8	3.5	3	2
Italy	3	1.3	1	0.7
Switzerland	5	2.2	5	3.4
Sweden	9	3.9	4	2.7
Argentina	3	1.3	1	0.7
Australia	2	0.9	2	1.3
Austria	2	0.9	-	-
China	2	0.9	-	-
Canada	1	0.4	1	0.7
Finland	1	0.4	-	-
Ireland	1	0.4	-	-
Netherlands	2	0.9	-	-
Portugal	1	0.4	-	-
S Africa	1	0.4	-	-
Pakistan	1	0.4	-	-
Czechoslovakia	1	0.4	-	-
Norway	1	0.4	-	-
Belgium	3	1.3	2	1.3
Denmark	3	1.3	2	1.3
TOTAL	229	100	149	100

Source: CRS Calculations

Note: The larger number of persons in the "citizen" column than in the "country of work" column reflects the fact that many of the "citizens" were honored in 1945 or later for work done before 1945.

Second, the Nobel Prize awards that have been made in the last few years typically recognize work done 10 or 20 years earlier. The average delay from the midpoint year of the completion of a Nobel Prize project until the receipt of a Nobel Prize is about 16 years, and, indeed, as Dr. Garfield said, it's a bit longer in chemistry than it is in other fields.

CHART 2

AVERAGE NUMBER OF YEARS BETWEEN PERFORMANCE OF CITED WORK AND RECEIPT OF THE NOBEL PRIZE FOR ALL SCIENCE LAUREATES SINCE 1945

Field	Method of Calculation	
	From Midpoint of Work	From End of Work
Physics	15.3	12.4
Chemistry	17.6	12.5
Physiology/ Medicine	15.3	11.2

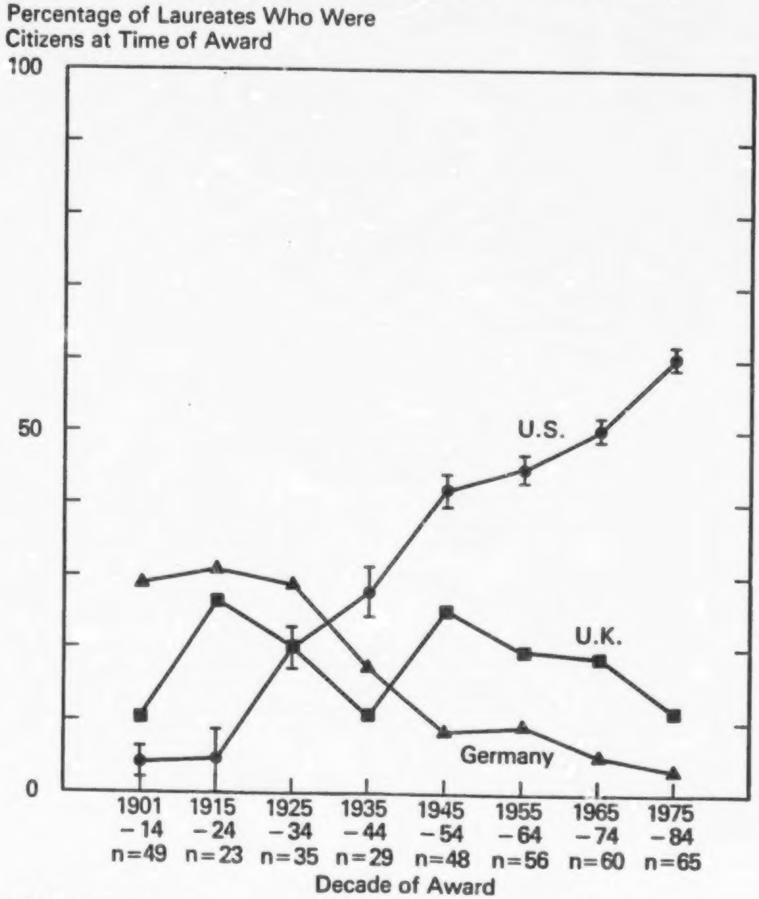
Source: CRS calculations

Of the 229 people who have received Nobel Prizes since 1945, only 31 have received them for work done since 1965, and only 4 for work done since 1975. There's no evidence for a systematic change over the last four decades in this average delay period. Because of this long delay the recent receipt of prizes by citizens of a country, we conclude, provides no information about the strength of its scientific community at the current time. At best, the patterns of current awards provide information on the relative strengths of national scientific efforts 10 or 20 or more years ago.

Citizens of the United States have earned a steadily increasing fraction of all the prizes each decade, rising from 42 percent in the first decade, to 60 percent in 1975 to 1984, and averaging 50 percent over the last four decades, as shown in the third chart.

CHART 3

Fraction of Science Nobel Prizes Awarded to Citizens of Major Countries 1901-1984



Note: Vertical Lines Show Effect of Winning One Prize More or Less in Each Decade.

Source: Data in Table 3

That chart, by the way, covers the entire 84-year period since the Nobel Prizes were established. It shows the gradual decline of Germany over that long period of time and the rapid rise in the United States.

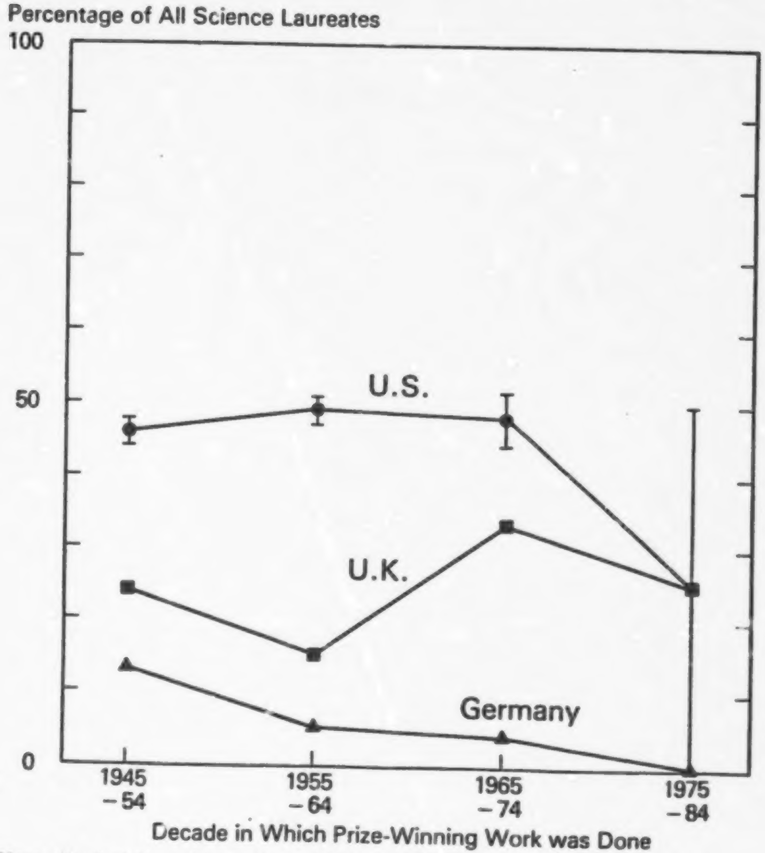
When viewed on the basis of the country in which the research was done, the United States actually ranks even higher. We have hosted an even larger fraction of the prize-winning work, 57 percent. But on a per capita basis, not a per-scientist basis—because we didn't have data on the number of scientists working in each country, but on a per capita basis, the United States does not rank first, we rank third. We're well behind Switzerland and the United Kingdom and just ahead of Sweden on a per capita basis in hosting Nobel Prize-winning science. One can well imagine that Switzerland is a bit of an anomaly because it has the CERN facility which raises their ranking perhaps rather artificially high.

Immigration has been an important source of U.S. Nobel Prize winners. Nearly a third of the 114 Nobel laureates honored since 1945, while U.S. citizens, were born overseas. But almost all of these, 111 out of the 114, did their prize winning work in the United States. That is to say, most of the immigrant Nobel Prize winners did the work for which they got the prize here, and 79 percent of them did their graduate education here. Only 7 of the 114 United States citizen winners since 1945 were born in Germany, and all 7 of those did their prize winning research in the United States.

During each of the first three decades after World War II, about half of all the winners of Nobel prizes received their graduate education in the United States. There's an early indication that this fraction may have declined during the most recent decade, but the number of winners for work done in the last decade is so small that we don't put much stock in this finding. The status of the United Kingdom as a country of graduate education for Nobel laureates has gone up somewhat over the decades while Germany's status has steadily declined, as shown in the fourth chart.

CHART 4

Fraction of All Nobel Laureates in Science Who Earned Their Highest Degree in Major Countries and Were Honored for Work Done Since 1945



Note: Vertical Lines Show Effect of Winning One Prize More or Less in Each Decade.

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Now I want to turn to our findings on the relationship of Nobel Prize winning to other indicators of national scientific and technical standing.

There are no absolute indicators of national scientific standing. If there were, we wouldn't be here this morning, we wouldn't have to discuss this vexing question. But we can get some insight by comparing the Nobel Prize indicators with other ones.

First, we found no correspondence between Nobel Prize winning and the amount of money spent on research and development in a country or the number of scientists and engineers who are working there and doing research and development. That is to say, simply doing more R&D does not guarantee that you win more Nobel Prizes.

Second, when the number of papers each country contributes to the world's scientific literature is compared with the pattern of Nobel Prize winning in related fields, we get a mixed, equivocal picture. Nations that publish more papers tend to host more Nobel Prizes, but there are important exceptions, both within specific fields and for countries on the whole. Let me say parenthetically that the data on which this is based originate with Dr. Garfield's organization.

One indicator of the average quality of a field of science in a country is the average frequency with which its published papers are cited. We found a significant relationship of national Nobel Prize winning and what's called the relative citation frequency, even when we corrected for the fact that larger bodies of literature are inherently likely to be cited more frequently. But, again, there's a wide variation among countries and fields around this general observation.

One measure of the technological standing of a nation is its share of world trade in high-technology manufactured goods. We found a reasonably strong correlation of Nobel Prize winning in the two fields of physics and chemistry with the share of Free World high-technology trade, as well as with the rate of increase of such shares from 1965 to 1982. But a very noticeable exception to this finding is Japan, which ranks very low in Nobel Prize winning but very high in high-technology trade share, as well as very high in the rate of growth of its high-technology trade share.

Another widely used measure of technological capability is the fraction of U.S. patents awarded to a nation's citizens. We found no correlation at the national level between patenting and Nobel Prize winning in physics and chemistry.

Now let's turn to the relationship of Nobel Prize winning to some larger national goals. An important motivation for U.S. Government support of science since the Bush Report has been the conviction that national strength in science is a key to reaching such societal goals as economic growth, public health, and national security.

Thus, it was obvious and natural to try to find out whether the Nobel Prize indicators are related to typical indicators of public health and economic status. We don't have a quantitative indicator of national security, and so we couldn't do any analysis related to that important goal of society.

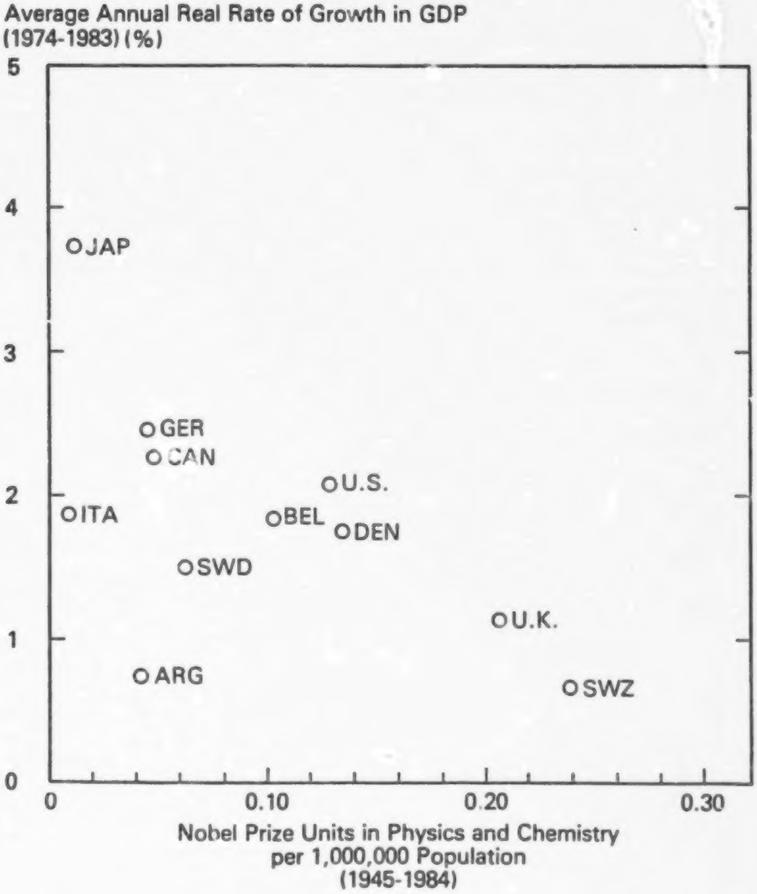
As indicators of national public health status, we examined infant mortality rates, life expectancies at age 1, age 15, and I think age 65, and rates of improvement in these measures over time.

The evidence we looked at does not support the notion that doing Nobel Prize winning research in physiology and medicine in a country gives the country a comparative advantage over other countries in terms of public health. We don't mean that Nobel Prize winning research confers no health benefits on mankind as a whole. For it assuredly does. Instead, we are saying that such research confers little or no measurable extra health benefit on the country that hosts it. This finding seems reasonable, in retrospect, in light of the rapid dissemination of developments in medical science around the world, and in light of the fact that a nation's public health status depends more critically on such factors as nutrition and lifestyle, and on the implementation of sanitation and medical care measures than it does on doing medical research within the country, per se.

Our findings regarding national economic performance are quite contrary to the views of some people. Among the countries that have hosted Nobel Prize winning physics and chemistry since 1945, there is a tendency for the nations with fewer prizes per capita to have experienced greater average rates of growth of both gross domestic product and labor productivity in manufacturing.

CHART 8

Growth in Gross Domestic Product and Nobel Prize-Winning in Physics and Chemistry



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I should point out that this is not just a finding that's related to Japan at one extreme or to the United Kingdom at the other, but all the countries that we looked at as shown in chart 8 line up on a reasonably strong straight-line correlation. We do not conclude that winning Nobel Prizes is bad for the economy, but our data do show that hosting Nobel Prize winning research in a country does not guarantee that it will have a strong economy.

It's well known that national economic performance is influenced by many factors other than doing first-rate science, including the health of national institutions, the development of new technology, and the investment in people and machines. Some have argued that some of our economic and competitiveness difficulties may reflect, however, an over-emphasis on supporting frontier research by a scientific elite coupled with an underemphasis on imparting basic mathematical and technical skills to all our workers and citizens. This suggests that there may be a tradeoff between focusing on the frontiers of science and achieving the national goals to which that science is directed.

I'd like to close with a few comments on the implications of our study of Nobel Prize winning for the broader theme of these hearings—that is, establishing optimal levels of investment in science.

Nobel Prize winning represents the very highest levels of achievement in fundamental science, and I hope it's well understood that we've taken that as a basic assumption. Individual discoveries for which prizes have been given are the foundation of many of the new technologies that have contributed to the betterment of mankind nearly everywhere. Perhaps the failure of Nobel Prize winning science to create comparative advantage for nations is telling us something very important about the support of research and about the nature of our science policy.

One of the fundamental rationales for the public support of science in a market economy is that the incentives for individuals and firms to invest in basic scientific research are undermined by the fact that those who do not pay for the research are nevertheless able to benefit from its results. Another case of—if you'll excuse me, Congressman—"Let George do it" is the best way to get it done. As the world's nations become more interdependent, and as they are brought closer together by new communications and transportation technologies, the lack of appropriability of scientific findings has become an international phenomenon. Typically, new scientific discoveries made in one country are rapidly made available for application nearly everywhere. For example, Dr. Garfield mentioned that he has observed as many as 150 citations within 1 year to certain papers in certain fields.

In these circumstances, it may no longer make sense to think of basic research solely as a national investment or to search for the optimum level of each nation's R&D spending in isolation. Instead, there is a real possibility that most nations responding to the rapid international leakage of science will invest less in basic research in the hope that the other nations will pick up the burden.

It might be good to remember that, prior to World War II, it was the practice of the United States to let Europe do basic research, and, until very recently, since World War II it has been the practice of Japan to let the United States and Europe pick up the burden of supporting basic research. But if most nations were to "Let George do it," the worldwide investment in R&D might fall to far less than optimal levels. Thus it may be useful to consider how to formulate an international strategy for supporting optimal levels of basic research in concert with considering how our own national science and technology strategy can assure that all of the world's new scientific knowledge is made available for effective application in the United States. Thus, for example, we may wish to expand our notions of international cooperation from a few large and expensive projects to a much wider array of scientific studies, and we may wish to consider a very considerable increase in the proportion of our national science and technology resources that we devote to finding out and disseminating what scientists in other nations have already learned.

Thank you, Mr. Chairman. We will be pleased to answer your questions.

[The prepared statement of Dr. Hill and Ms. Winston follows:]

Congressional Research Service
The Library of Congress

Washington, D.C. 20540

THE NOBEL-PRIZE AWARDS IN SCIENCE AS A MEASURE OF NATIONAL STRENGTH IN SCIENCE

Testimony at the Request of the
Science Policy Task Force
Committee on Science and Technology
U.S. House of Representatives
April 16, 1986

Christopher T. Hill
Senior Specialist
Science and Technology Policy

and

Joan D. Winston
Senior Research Assistant
Office of Senior Specialists

CONTENTS

REASON FOR THE STUDY.. 1

APPROACH USED IN THE STUDY.. 2

NATIONAL STANDING IN NOBEL-PRIZE WINNING.. 5

RELATIONSHIP OF NOBEL-PRIZE WINNING TO OTHER INDICATORS OF NATIONAL SCIENTIFIC AND TECHNOLOGICAL STANDING.. 7

RELATIONSHIP OF NOBEL-PRIZE WINNING TO LARGER NATIONAL GOALS........... 9

CLOSING COMMENTS.. 10

Mr. Chairman, and Members of the Task Force. Soon after its establishment, the Science Policy Task Force requested that the Congressional Research Service do a study of the usefulness of the Nobel-Prize awards in science as a measure of national strength in science. We are here today to report to you on the findings of that study.

REASON FOR THE STUDY

The Nobel Prizes in physics, in chemistry and in physiology or medicine, which have been awarded to outstanding individuals since 1901 under the terms of the will of Alfred B. Nobel, are the most prestigious and well-known of all of the awards offered to scientists for their contributions to science and to the welfare of humanity.

Since their founding, citizens of the United States have received an increasing share of these prizes in all three fields. In the last decade U.S. citizens have taken 60 percent of them. The only nation that has approached the United States in garnering Nobel Prizes in the sciences since World War II is the United Kingdom, whose citizens have won 11 percent of the Prizes in the last decade.

While the Nobel Prizes are awarded to individuals, the number of prizes won by a nation's citizens is frequently used as an indicator of that country's relative strength in science. Furthermore, the national patterns of Nobel-Prize winning are often regarded as indicators of national technological capability, of national capacity for vibrant economic performance, and of national determination to improve the health of the citizenry.

For example, on the first page of its recently released Biennial Science and Technology Report to the Congress: 1983-1984, the Office of Science and Technology Policy states:

> The importance of science and technology to our national purpose has been recognized since the era of Franklin and Jefferson. Yet today, more than ever before, our economic competitiveness, our national security in its broadest sense, and our ability to provide better health and quality of life for all our citizens depend on the strength and vitality of our science and technology enterprise.... The U.S. science and technology enterprise still leads the world. Scientists resident in this country have won or shared 46 Nobel Prizes during the past 10 years in physics, chemistry, physiology/medicine, and economics.

Questions are raised from time to time, however, about the significance of the fact that the United States gets so many of the prizes. Many believe that the U.S. success should be attributed as much to the fact that it has been the homeland of choice of many expatriate European scientists as to the quality of its indigenous research and development community. And, some have noticed that the rank order of nations by their success in winning Nobel Prizes is not the same as the ranking of the same nations according to other measures of scientific, technological and economic performance.

The purpose of our study was to provide a more solid empirical foundation for understanding the significance of national patterns of Nobel Prize winning.

APPROACH USED IN THE STUDY

Our study for the Task Force was based on an empirical analysis of the 229 winners of the Nobel Prizes in the natural sciences from 1945 through 1984. We excluded from our study the prizes for literature and for peace, as well as the more recently established Alfred Nobel Memorial Prize in economic sciences.

The national patterns of Nobel-Prize winning were compared with other measures of national scientific strength such as R&D spending, scientific manpower, and scientific publication. The Nobel-Prize performance of each country was also compared with indicators of its technological competitiveness, economic performance, and health status to determine whether success in prize winning is related to these other national goals.

This is not a report on the sociology of the Nobel-Prize winners, nor is it a study of the individual lives and achievements of this outstanding group of men and women. We assumed that the Nobel laureates have been recognized for scientific work of the highest quality and importance, and that they are worthy of the honors bestowed on them. These assumptions are widely shared both within and outside the scientific community, and are supported by a host of other evidence that the individual Nobel-Prize winners are outstanding contributors to science.

Finally, we wish to emphasize that the study focuses on whether Nobel-Prize-winning research contributes to the comparative advantage of the nation in which the research is done. We did not attempt to determine whether such research contributes to the improvement of the entire group of prize-winning nations or of the world as a whole.

Several details of the Nobel Prizes must be addressed in understanding the significance of the national patterns of award. First, each prize is sometimes divided among as many as three persons whom the committees decide deserve prizes in that year. The winners may have cooperated on the same work, may have made the same discovery simultaneously, or may have made entirely unrelated discoveries. Divided prizes are always split quantitatively into halves, into one half and two quarters, or into three thirds.

Second, the prizes are officially denominated by the year in which the award is made and by the nation of which the recipient is a citizen at the time the award is made. Since a substantial number of the winners have emigrated one or more times during their lives, the attribution of Nobel-Prize-winning work to nations on the basis of the official country of award can be misleading. Also, there is usually a delay of 10 to 20 years or more from the time at which prize-winning work is done until it is recognized by the Nobel committees. This further complicates the interpretation of the time series of the awards to various nations.

Finally, the time period over which the cited research of the Nobel laureates is performed can vary greatly. Some do their prize-winning work in a single year, the work of others is carried out over a long period of time and comes to fruition only after many years of preparation or false starts, and for yet others the cited work is a series of contributions made over a number of years. The result is that there is considerable ambiguity in attributing the prize-winning work to a specific year.

In constructing indicators of the Nobel-Prize-winning performance of each country, we assumed that:

1. Each prize was assigned a value of one prize unit.
2. When a prize was divided, each winner was assigned the appropriate fraction of a prize unit.
3. The date of each prize was the mid-point year of the period in which the prize-winning work was done, not the year of the award.
4. Each prize was assigned to the country in which the prize-winning work was done, not to the country of citizenship of the winner.

In order to construct these indicators, biographical data on all of the 229 winners since 1945 were assembled and entered into a computerized data base. All of that data is in an appendix to our report to the Task Force.

We divided the study period 1945 to 1984 into four decades, and based our standard indicator of national Nobel-Prize-winning performance on the number of prize units accumulated in each decade by scientists working in each country. For some purposes, the standard indicators were divided by the population of each country in 1970 to account for country size.

Since we completed our study before the 1985 prizes were announced, those five laureates are not considered in our results. However, adding them to our study population would have made negligible changes in our findings.

NATIONAL STANDING IN NOBEL PRIZE WINNING

Let us summarize a few of our findings on national patterns of Nobel Prize winning.

First, the rank order of the countries that have received a substantial number of Nobel Prizes is not substantially affected by the details of construction of the indicators of national Nobel-Prize achievement. However, some countries whose citizens have won Nobel Prizes since 1945 for work they did before 1945 or for work they did while they were resident in other countries do not appear on the list of countries that have hosted Nobel-Prize-winning research since 1945. Whereas the citizens of 24 countries have been awarded prizes since 1945, only 14 countries have actually hosted prize-winning work since that year. (chart 1)

Second, the Nobel Prize awards that have been made in the last few years typically recognize work done 10 or 20 years earlier. (chart 2) Of the 229 persons who have received Nobel Prizes since 1945 in the three fields of science, only 31 have received them for work done since 1965, and only 4 for work done since 1975. There is no evidence for a systematic change over the last four decades in the average delay in the recognition of Nobel-Prize-winning research.

Because of this long delay in the recognition of outstanding contributions, the recent awarding of prizes to citizens of a country provides no information about the strength of its scientific community at the current time. At best, current awards provide information on the relative strengths of national scientific efforts 10, 20, or more years ago.

Citizens of the United States have earned a steadily increasing fraction of all the prizes each decade, rising from 42 percent in 1945-1954, to 60 percent in 1975-1984, and averaging 50 percent over the four decades. (chart 3) When viewed on the basis of the country in which the research was done, the United States has hosted an even larger fraction of the prize-winning work over the four decades: 57 percent. On a per capita basis, however, the United States ranks third, well behind Switzerland and the United Kingdom and just ahead of Sweden in hosting Nobel-Prize-winning science.

Immigration has been an important source of the U.S. Nobel Prizewinners. Nearly one-third of the 114 Nobel laureates honored since 1945 while U.S. citizens were born overseas. However, 111 of the 114, or 97 percent, did their prize-winning work in the United States, and 79 percent of them did their graduate education here. Contrary to popular perception, only 7 of the 114 U.S.-citizen winners since 1945 were born in Germany, and all 7 did their prize-winning research in the United States. On the other hand, of course, many distinguished scientists did emigrate to the United States around the time of World War II from Germany and other parts of Europe, and a number of them had already won Nobel Prizes or were widely considered to be prime candidates for a prize.

During each of the first three decades after World War II, about half of all the winners of Nobel Prizes for work done since 1945 received their graduate education in the United States. However, there is some indication that this fraction may have gone down in the most recent decade. The status of the United Kingdom as the country of graduate education for Nobel laureates has gone up somewhat over the decades while Germany's status has steadily declined. (chart 4)

RELATIONSHIP OF NOBEL-PRIZE WINNING TO OTHER INDICATORS OF NATIONAL SCIENTIFIC AND TECHNOLOGICAL STANDING

There are no absolute indicators of national scientific standing with which we can compare the Nobel-Prize indicators. However, we can get some insight by comparing Nobel-Prize winning with other science and technology indicators.

Except for the fact that the United States is far ahead of the rest of the world on each measure, there is no correspondence between Nobel-Prize winning and the amount of money spent on research and development in a country or the number of scientists and engineers working there in R&D. Thus, simply doing more R&D does not guarantee earning more prizes.

When the number of papers each country contributes to the world's scientific literature is compared with the patterns of Nobel-Prize winning in related fields, a rather equivocal picture emerges. (chart 5) Nations that publish more tend to host more Nobel Prizes, but there are important exceptions to this tendency, both within specific fields of science and for countries on the whole.

One indicator of the average quality of a field of science in a country is the average frequency with which its published papers are cited. There is a significant relationship between national Nobel-Prize winning and the relative citation frequency, even when one corrects for the fact that larger bodies of literature are inherently likely to be cited more frequently. However, while more frequently cited nations tend to host more Nobel Prizes, there is also a wide variation among countries and fields around this general observation. (chart 6)

One measure of the technological standing of a nation is its share of world trade in high-technology manufactured goods. We found a reasonably strong correlation of Nobel-Prize winning in physics and chemistry with the share of free-world high-technology trade, as well as with the rate of increase of such shares from 1965 to 1982. This relationship holds up even when we corrected for the fact that larger economies tend to have larger world-trade shares. However, a notable exception to this finding is Japan, which ranks very low in prize-winning but high in high-tech trade share, as well as very high in the rate of growth of its high-tech trade share over the last two decades.

Another widely used measure of technological capability is the fraction of U.S. patents that is awarded to a nation's citizens. However, we found no correlation at all at the national level between patenting and Nobel-Prize winning in physics and chemistry.

RELATIONSHIP OF NOBEL-PRIZE WINNING TO LARGER NATIONAL GOALS

An important motivation for U.S. Government support of science since the Bush report of 1945 has been the conviction that national strength in science is a key to reaching such societal goals as economic performance, public health and national security.

In view of the strength of this conviction, and in view of the widely held belief that winning Nobel Prizes is a useful indicator of the nation's ability to reach these larger goals, we decided to try to find out whether the Nobel Prize indicators are related to typical indicators of public health and economic status. In the absence of an accepted indicator of comparative national security, we did not examine the relationship to this goal.

As indicators of national public health status, we examined infant mortality rates, life expectancies at various ages, and rates of improvement in these measures over time.

The evidence examined for this study does not support the notion that doing Nobel-Prize-winning research in physiology and medicine in a country gives the country a comparative advantage over other countries in terms of public health. (chart 7) This is not to suggest that Nobel-Prize-winning research confers no health benefits on mankind as a whole, for it assuredly must. Instead, we are saying that such research confers no measurable extra health benefit on the country that hosts it. This finding seems reasonable in light of the rapid dissemination of developments in medical science around the world, and in light of the fact that a nation's public health status depends more critically on such factors as nutrition and life style, and on the implementation of sanitation and medical-care measures, than on doing medical research per se.

Our findings regarding national economic performance are quite contrary to the conventional view. Among the countries that have hosted Nobel-Prize-winning physics and chemistry since 1945, there is a tendency for the nations with fewer prizes per capita to have experienced greater average rates of growth of both gross domestic product and labor productivity in manufacturing. (chart 8) While we do not conclude that winning Nobel Prizes is injurious to an economy, our data do show that hosting Nobel-Prize-winning research in a country does not guarantee that it will have a strong economy.

It is well known that national economic performance is influenced by many factors other than doing first-rate scientific research, including the health of national institutions, development of new technology, and investment in people and machines. Indeed, it has been argued that some of the economic and competitiveness difficulties of the United States may reflect and overemphasis on supporting frontier research by the scientific elite, coupled with an underemphasis on imparting basic mathematical and technical skills to all workers and citizens.

CLOSING COMMENTS

In summary, the hosting of Nobel-Prize-winning science in a country is most closely correlated with indicators of citations to its published scientific literature. This is a reasonable finding since both reflect the quality of a nation's scientific contributions. Prize winning is much less closely correlated with measures of the quantity of a nation's science as measured by financial and human resources devoted to R&D or by the quantity of its scientific publications. Prize-winning is only weakly associated with measures of technological performance. There is no correlation between national public health status and Nobel-Prize winning in physiology or medicine. And, there is actually a negative correlation between national economic performance and the hosting of Nobel-Prize-winning research in physics and chemistry in the country.

Finally, Nobel-Prize winning represents the very highest levels of achievement in fundamental science. Individual discoveries for which prizes have been given are the foundation of many of the new technologies that have contributed to the betterment of mankind nearly everywhere. Perhaps the failure of Nobel-Prize-winning science to create comparative advantage for nations is telling us something very important about the support of research.

Recall that one of the fundamental rationales for the public support of science in a market economy is that the incentives for individuals and firms to invest in basic scientific research are undermined by the fact that those who do not pay for the research are nevertheless able to benefit from its results. That is, the benefits of the research are not fully "appropriable." As the world's nations become closer and more interdependent and as they are brought closer together by new communications and transportation technologies, the lack of appropriability of scientific findings has become an international phenomenon. Typically, new discoveries made in one country are rapidly made available for application nearly everywhere.

In these circumstances, it may no longer make sense to think of basic research solely as a national investment, or to search for the optimum level of each nation's R&D spending in isolation. Instead, there is the real possibility that most nations, responding to the rapid international "leakage" of science, will invest less in basic research in the hope that other nations will pick up the burden. But, if most nations were to follow this route, the world-wide investment in R&D might fall to far less than optimal levels. Thus, it may be useful to consider how to formulate an international strategy for supporting optimal levels of basic research, in concert with considering how our national science and technology strategy can assure that all of the world's new scientific knowledge is made available for effective application in the United States.

CHART 1

NATIONAL STANDINGS IN SCIENCE NOBEL PRIZE ACHIEVEMENT

1945-1984

Country	Number of Prize Winners Among Country's Citizens		Number of Prize Winners Who Did Winning Work in the Country Between 1945-1984	
	#	% of Total	#	% of Total
U.S.	114	50	85	57
U.K.	41	18	30	20
Germany	14	6	7	4.7
France	6	2.6	3	2
Japan	4	1.7	3	2
U.S.S.R.	8	3.5	3	2
Italy	3	1.3	1	0.7
Switzerland	5	2.2	5	3.4
Sweden	9	3.9	4	2.7
Argentina	3	1.3	1	0.7
Australia	2	0.9	2	1.3
Austria	2	0.9	-	-
China	2	0.9	-	-
Canada	1	0.4	1	0.7
Finland	1	0.4	-	-
Ireland	1	0.4	-	-
Netherlands	2	0.9	-	-
Portugal	1	0.4	-	-
S Africa	1	0.4	-	-
Pakistan	1	0.4	-	-
Czechoslovakia	1	0.4	-	-
Norway	1	0.4	-	-
Belgium	3	1.3	2	1.3
Denmark	3	1.3	2	1.3
TOTAL	229	100	149	100

Source: CRS Calculations

Note: The larger number of persons in the "citizen" column than in the "country of work" column reflects the fact that many of the "citizens" were honored in 1945 or later for work done before 1945.

CHART 2

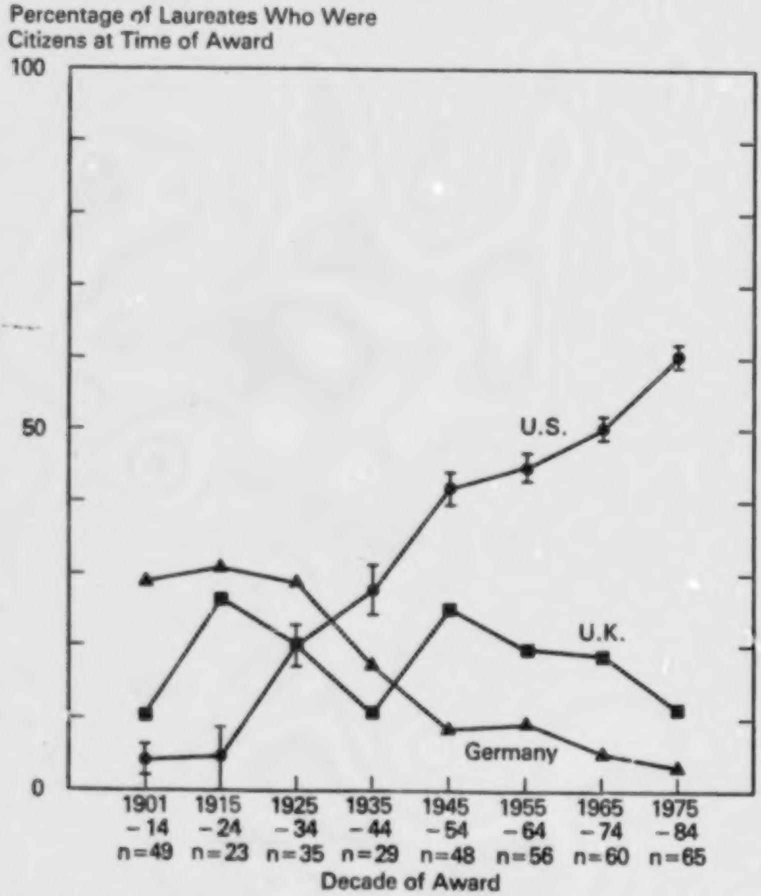
AVERAGE NUMBER OF YEARS BETWEEN PERFORMANCE OF CITED WORK AND RECEIPT OF THE NOBEL PRIZE FOR ALL SCIENCE LAUREATES SINCE 1945

Field	Method of Calculation	
	From Midpoint of Work	From End of Work
Physics	15.3	12.4
Chemistry	17.6	12.5
Physiology/ Medicine	15.3	11.2

Source: CRS calculations

CHART 3

Fraction of Science Nobel Prizes Awarded to Citizens of Major Countries 1901-1984



Note: Vertical Lines Show Effect of Winning One Prize More or Less in Each Decade.

Source: Data in Table 3

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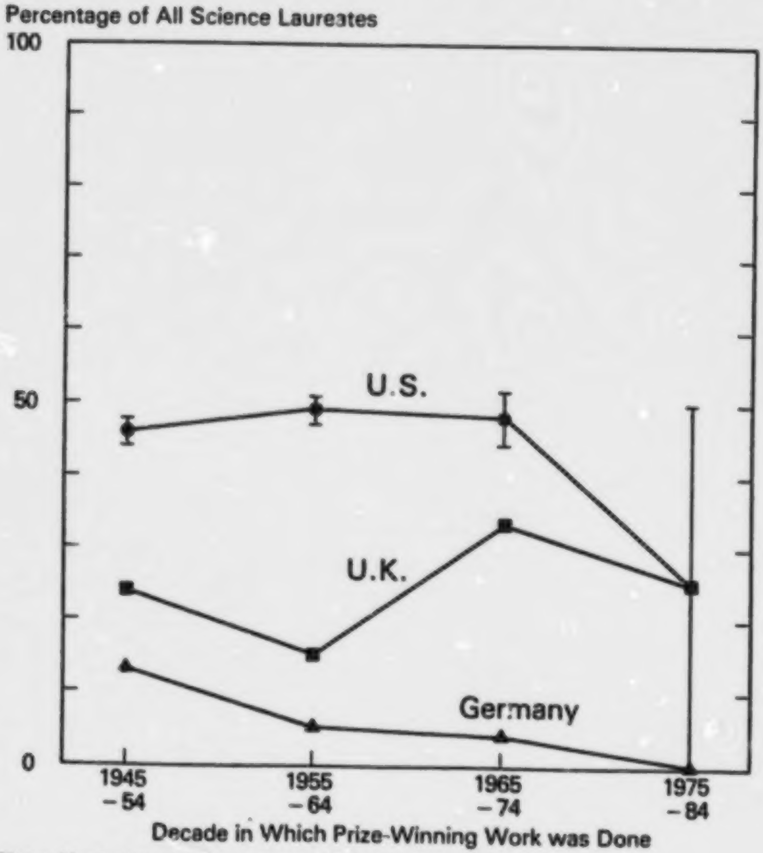
CHART 4

Fraction of All Nobel Laureates in Science Who Earned Their Highest Degree in Major Countries and Were Honored for Work Done Since 1945



Note: Vertical Lines Show Effect of Winning One Prize More or Less in Each Decade.

CHART 5

A Comparison of Nobel-Prize Winning and Scientific Publication *

Country	Physics		Chemistry		Physiology & Medicine	
	Percentage of 1973 Publications	Percentage of Nobel-Prize Units, 1945-1974	Percentage of 1973 Publications	Percentage of Nobel-Prize Units, 1945-1974	Percentage of 1973 Publications **	Percentage of Nobel-Prize Units, 1945-1974
U.S.	32.7	71.2	23.4	37.9	41.5	62.9
U.K.	7.7	13.7	7.8	34.7	10.0	18.2
W. Germany	5.4	3.2	6.1	8.4	6.5	2.1
Switzerland	NA	0	NA	2.1	NA	3.5
Sweden	0.9	2.2	1.0	0	2.4	4.2
Belgium	NA	0	NA	4.2	NA	1.4
Japan	6.5	2.5	9.4	2.1	3.6	0
Argentina	NA	0	NA	4.2	NA	0
Canada	3.7	0	3.9	4.2	3.8	0
France	5.3	0	6.7	0	6.2	4.2
U.S.S.R.	18.4	4.3	16.4	0	4.4	0
Australia	1.2	0	1.6	0	2.1	3.5
Denmark	NA	2.9	NA	0	NA	0
Italy	2.1	0	2.3	2.1	1.8	0
E. Germany	0.5	0	1.5	0	0.8	0
India	3.7	0	3.9	0	1.4	0
New Zealand	0.1	0	0.4	0	0.3	0
S. Africa	0.2	0	0.3	0	0.7	0
Israel	0.1	0	0.7	0	1.0	0

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* Based on unpublished analysis of the ISI database by Computer Horizons, Inc., Cherry Hill, N.J., and supplied courtesy of the National Science Foundation.

** Average of the values for clinical medicine and biomedical research.

CHART 6

Relationship of Relative Citation Ratio to Nobel-Prize Winning in Three Fields of Science. (●) Physics, (■) Chemistry, (▲) Biology.

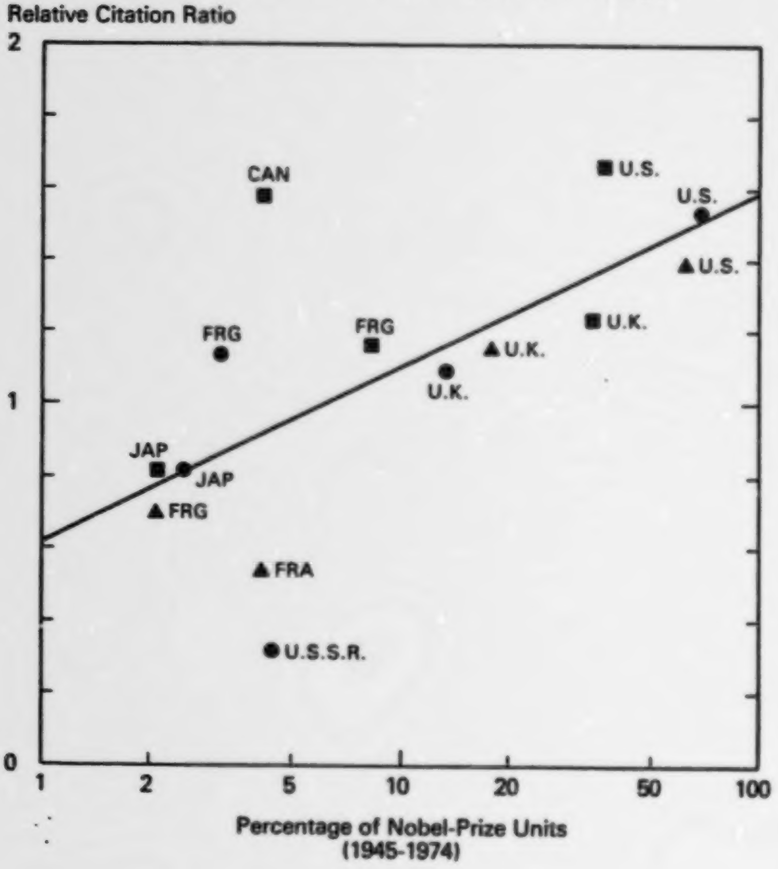

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CHART 7

Infant Mortality and Population-Weighted Nobel Prize-Winning in Physiology or Medicine

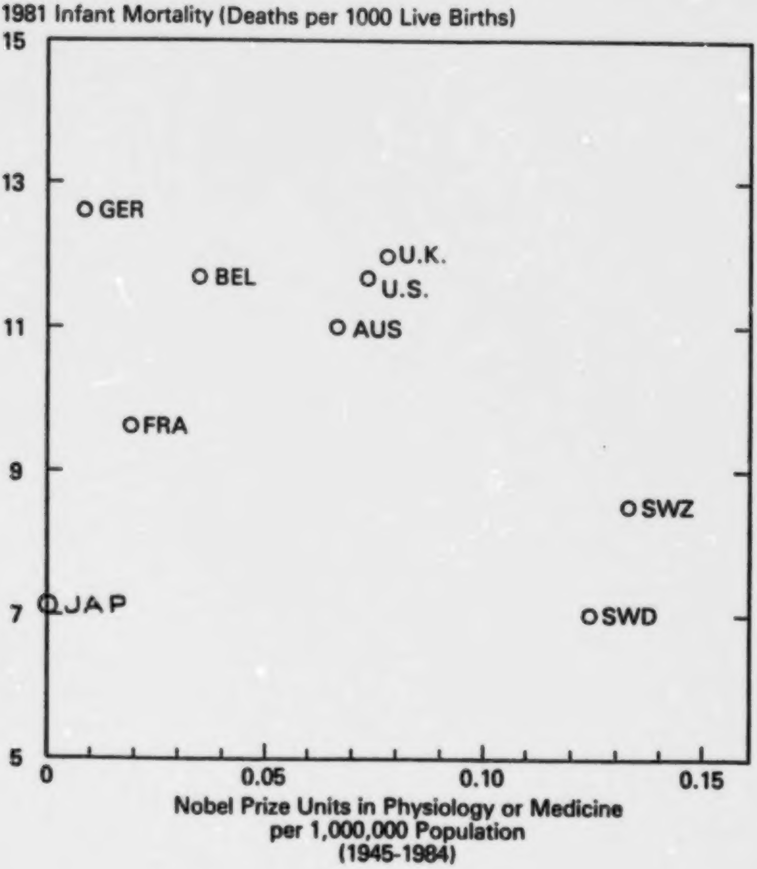

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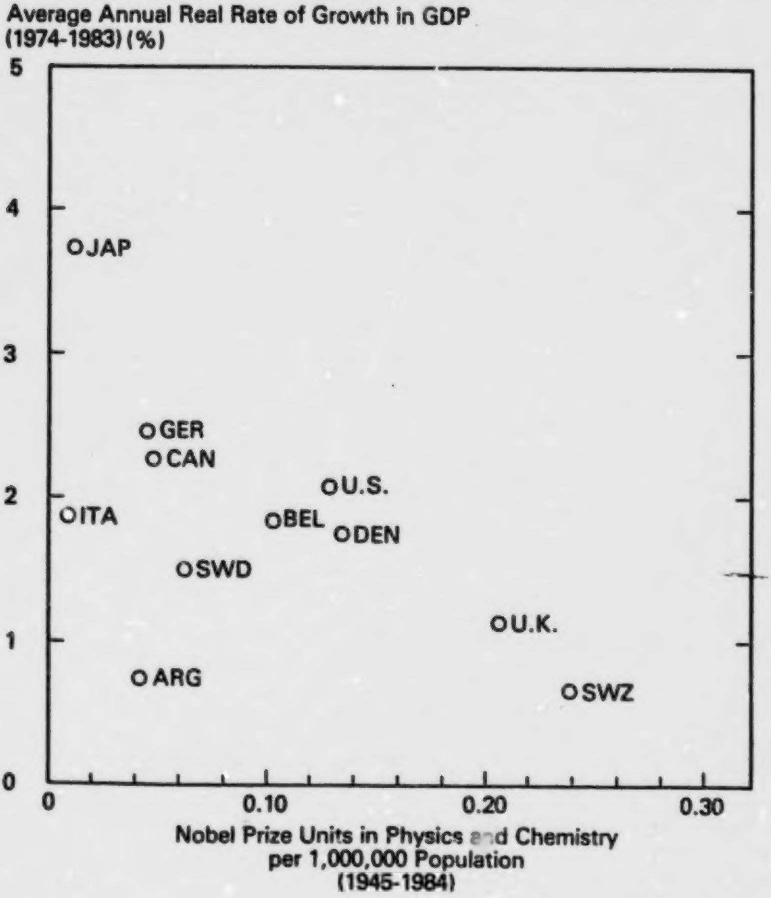
CHART 8

Growth in Gross Domestic Product and Nobel Prize-Winning in Physics and Chemistry



DISCUSSION

Mr. **Fuqua**. You note that about one-third of the 114 Nobel laureates since 1945 came from abroad. Is there any relevance in that finding to the current debate in this country on the issue of the number of foreign nationals in the graduate schools that we have at many of our science and engineering facilities? Is there a significant difference between the influx that we're experiencing today and the influx that we had back in the 1930's and 1940's?

Dr. **Hill**. I think perhaps the only significant difference is the country of origin of a large number of the people who are now coming here to study. There's been a shift in the countries from Europe to East Asia, and, of course, at one point we had a very large number of Middle Eastern students, particularly from Iran. But I think one conclusion one can draw is that the immigrant scientists—and, after all, as it's often said, almost all of us were immigrants at one point or another—have made enormous contributions to science in this country, and we can certainly view this at least as much as an opportunity as we can a problem for us.

Mr. **Fuqua**. Is there a relationship between a country winning the Nobel Prizes and a country having a strong basic research program, or can we say that there's a relationship between the strength of basic research and technological strength?

Dr. **Hill**. I think a direct answer to that is that we don't really have very good comparative data on basic research if by that we mean the amount of money spent on basic research in each country. In fact, there's very little comparative international R&D data until quite recently. Recall that we expect the technological performance or the economic performance to happen some years after the science is done. So it doesn't help us to look at today's levels of basic research in various countries if we're going to try to make some association with performance. We have to be able to look at basic research spending 20 years or more ago in view of this lag, and we simply didn't have the data that would allow us to compare basic research spending versus Nobel Prize winning.

If we take the number of scientific publications, as a reasonable measure of commitment to basic research, on the grounds that applied researchers and technology developers don't publish very much, then we do see, as we said, a mixed association between publication and Nobel Prize winning. There's certainly some relationship; how strong it is is somewhat in question.

Mr. **Fuqua**. What is the criteria for selecting a Nobel Prize winner, and is there a correlation between that and any statistical analysis that you do for, as Dr. Garfield said, the world-class or Nobel-class scientist?

Dr. **Hill**. I suppose the final criteria are known only to God and the Nobel committees, and that really is a very closely held, quiet process. There have been a number of studies of who decides, and how they decide, and whether it's important to be part of an old boy's network to be selected, and so on.

Perhaps the person to ask that question, is in fact, Dr. Garfield, who has done some interesting examinations of the relationship of the individual Nobel Prize winners to things like whether they were members of the National Academy of Sciences or whether they had been extraordinarily high publishers. These kinds of sociology of science studies suggest that the Nobel Prize winners, with a few exceptions, are at the very peak of their fields, no matter how you look at them: they publish a lot, they get cited a lot, they get lots of other awards before they get Nobel prizes, they tend to be elected to the academies before they get the prizes, and so on.

But, no. I seriously doubt whether the selection committees take into account the kind of statistical work that we've looked at.

It has been argued that in the 1930's and 1940's there may have been some international prejudices reflecting concern about whether scientists from the Nazi powers ought to be considered and that sort of thing, but beyond that, very little is known about the selection process, particularly about the last 40 or 50 years.

Mr. **Fuqua**. Mr. Brown.

Mr. **Brown**. Dr. Hill, I'm particularly interested in the findings that you report on page 10 which you say are quite contrary to the conventional views, and that's the correlation between national economic performance, and we have, I think, been vaguely aware that that's true just by looking at the rapid economic progress of Japan and its relatively low Nobel Prize winning status, and the relatively poor—and I don't want to downplay it too much—of the United States despite its very heavy prize winning.

Would you like to just speculate on what the missing link is here, why the economic performance doesn't correlate more closely with Nobel Prize winning? Of course, basically I'm looking for you to confirm my own prejudices, but if you don't happen to, why, it's all right, too.

Dr. **Hill**. As part of my response, let me refer you to page 19 of the testimony, which is the chart that shows these relationships. We can, if you like, tell stories about Japan, we can tell stories about the United Kingdom, we can tell stories about ourselves, in order to explain the special factors that influence each country's performance, but almost all of the countries line up on the same line.

I've been challenged by this, and I've said, "Well, I can tell you a story about each one of the countries that would tell you why I shouldn't have included it on the chart, but if I tell you a story about all of them, then we don't have any relationship left."

Now, to try to answer your question. First, I think it happens because economic performance depends on a lot of things besides doing basic research. As has been pointed out many times before this task force and committee, doing basic research by itself is, in fact, only a cost; it provides no benefit—other than the psychic benefits to those who perform it and enjoy the results—unless someone goes the next step, which is to apply that scientific understanding to the development of new technology, to investment in new plant and equipment, and to a better understanding of how people work so that we can enhance worker productivity, and so on. Therefore, I have to believe that the chart that I show you is in some sense accidental. Otherwise, I'm left with the conclusion that focusing on that very thin stratum of elite science and putting so much attention on Nobel Prize winning science hurts the economy, and I just can't bring myself to conclude that, that it's not good for us.

Mr. **Brown**. But you would have to conclude that it would be a fallacious argument to say that our economic progress depends upon how strongly we support basic science.

We can assume that there's a weak relationship between support of basic science and Nobel Prize winning, but there's no correlation between Nobel Prize winning and economic performance by any index we can measure. So the assumption must be that there's very little relationship between support for research—fundamental research in the sciences and economic performance.

Dr. **Hill**. Let me try to qualify that by reminding you that we're only looking at 14 countries, and they're all rich countries. If we were to put Zaire, and Paraguay, and Malaysia on the same chart, then we'd come to a totally different conclusion. In other words, it is surely good in some larger sense for a nation to do basic research and to develop the manpower that can apply that research and so on. So, we are looking only at the competition among the rich countries, if you like, and who's ahead in that competition.

But if we compare the status of the rich countries with the poor countries, if I can divide them up that way, you're clearly better off, it seems to me with a strong scientific and technical base, including the support of basic research. But I think it also suggests that basic research is not the only thing you should be doing, and there's surely some limit beyond which doing more and more basic research is not the right way to be going.

Mr. **Brown**. Well, I'm not trying to make an argument that supporting basic research isn't good; I think it's an unadulterated good. I'm trying to clarify the arguments we use to justify it. I'm trying to elicit here whether the argument that it's a strong contributor to economic performance is the most valid argument to use, and I don't quite see that your data would substantiate that.

Dr. **Hill**. I think what doing basic research in the country does mostly for you is to give the scientific and technical communities in the country, well, two things. First, if the United States is doing something on the order of 40 or 50 percent of all the world's basic research, then we're still contributing a lot to ourselves as well as to everyone else, and that's of value to everyone. Second, the fact that we do that basic research makes us able to understand what the other folks are doing, and it also, in a way, trains the next generation of scientists and engineers.

My own judgment—this is not a CRS view on the issue—is that the kind of training that we offer in universities to do basic research is not the best kind of training to give to people who will do applied research technology development. The argument that basic research is essential to the training of the next generation of scientists and engineers, I think, has some large holes in it that we ought to ask ourselves about very carefully.

Mr. **Brown**. Well, the argument that you suggest here is the one that I've heard from many private corporations that support the research laboratories and fundamental research. They argue that this is necessary in order to provide for the training of their own people, provide them access to the frontiers of knowledge, and that even if it does not result in anything of commercial benefit to them, these other factors justify their doing it. I think we can extend that to the national level—these other factors justify our doing it—but if we're looking for immediate translation into econome progress, we've got to look at a broader range of factors, and my question has been here for a number of years, how do we get to that broader range of factors?

Thank you, Mr. Chairman.

Mr. **Fuqua**. Mr. Slaughter.

Mr. **Slaughter**. No questions.

Mr. **Fuqua**. Thank you very much, Dr. Hill, Ms. Winston. We appreciate you being here with us this morning.

Dr. **Hill**. Thank you for the opportunity to present our work.

[Whereupon, at 11:50 a.m., the task force was adjourned to reconvene on Tuesday, April 22, 1986 at 10 a.m.]

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APPENDIX

ADDITIONAL MATERIALS FOR THE RECORD

Executive Summary

ECONOMIC EFFECTS OF GOVERNMENT R&D SPENDING IN THE UNITED STATES

Nestor E. Terleckyj

Final Report to the National Science Foundation on the Project PRA 81-18098. NPA Discussion Paper 8-31-84. National Planning Association, 1606 New Hampshire Avenue, N.W., Washington, D.C. 20009, c1984.

I. Overview

This report presents the results of research supported in whole or in part by the National Science Foundation grant No. PRA 81-18098, "Measuring the Economic Impact of Government R&D." It contains a series of analyses exploring the effects of government R&D expenditures on productivity growth of the private economy and on private R&D expenditures. These analyses take into account the differences in the objectives of government programs that involve expenditures for research and development and attempt to identify the differences in their economic effects. The analyses consider the intercompany and interindustry impacts of government R&D as well as the interactions between government R&D programs and policies and private R&D choices. This report also presents an analytical model of the time pattern of the effects of private R&D investments on productivity growth in the economy and a series of empirical estimates based on this model.

II. Economic Analysis of Government R&D: A Survey

Previous economic research on government R&D activities has focused on the effects of government R&D on productivity growth in the private sector and on private R&D expenditures.

Generally, government R&D programs undertaken in support of productivity growth or technological development of an industry have been found to improve productivity of that industry. The classic case is the American agriculture. On the other hand, no direct effect has been found of government contract R&D on the productivity of firms or industries performing the R&D. But this R&D was undertaken in support of government programs, not in support of industry productivity. There is some evidence that productivity growth of industries buying producer goods from industries performing government contract R&D may have been somewhat enhanced, but this subject remains to be explored further.

With regard to the effect of government R&D on private R&D expenditures, there are strong indications that a direct positive relationship exists between the real volume of government contract R&D performed in industry and the real private industry expenditure on research and development. A number of different studies examined this relationship at the level of products or fields of application (Mansfield and Switzer, 1982; Scott, 1984), firms (Link, 1982), industries (Goldberg, 1979; Levin and Reiss, 1984), and economy (Levy and Terleckyj, 1983) and found such positive correlation. While negative correlation has been observed in some instances, (Carmichael, 1981), in most cases the relationship was found to be positive. The research performed at the level of the private business economy found that the total effect of government contract R&D on private R&D spending estimated at this level was considerably larger than the effects estimated by other researchers at the level of industries or companies. This suggests that there may exist considerable interindustry, intercompany and interproduct cross effects (technology transfer effects) of government contract R&D in stimulating private R&D outlays. Thus, following the original adaptation of the results of government contract projects to private market products further R&D expenditures by the original innovators may be induced as well as R&D spending by imitators and developers of subsequent applications of these results in other products and markets.

III. Effects of Government R&D on Private R&D Investment

The relationship between government contract R&D and private R&D expenditures was examined within the context of the macroeconomic U.S. investment function for private industry R&D expenditures. (Most of these results were published in Levy and Terleckyj, 1983.)

First, the total government expenditures for R&D were introduced into this function as an independent variable determining the private industry investment in R&D along with tax variables, output, unemployment, and age of the stock of the R&D capital. The coefficient for total government R&D in this investment equation was highly significant and had the value of .21. This can be interpreted as being consitent with the hypothesis that an additional 21 cents of private R&D outlays has been induced by one dollar of government R&D spending. However, further distinctions regarding the kind of government R&D expenditure proved to be important.

When total government R&D expenditure was separated into government contract R&D and all other government R&D (i.e., the R&D performed in government, in universities and in nonprofit institutions), a larger and significant coefficient was obtained for government contract R&D, at .28, while the coefficient for all other government R&D was very small and not statistically significant.

A search was conducted for time lags in this relationship. No lags could be identified for the government contract R&D performed in industry. However, when the data for other government R&D was lagged three years, a coefficient significant at a 10 percent level was found, which had the value of .19. The coefficient for government contract R&D remained virtually unchanged at .27 and stayed highly significant. Further disaggregation in other government R&D was not attempted.

It is possible that the strong correlation observed between government contract R&D and private R&D has been influenced by some procedures in the definition of R&D statistics. The government contractors, specifically those for the Department of Defense and NASA, are allowed to charge certain company expenditures for R&D as part of the overhead cost on all government contracts, including procurement as well as R&D contracts. This R&D expenditure appears in the NSF survey data as private R&D outlays when in effect it is paid for, indirectly, by the government. Such contractor reimbursement of R&D, also known as IR&D (independent R&D), as a normal cost of doing business originated in the early 1960s after government contracting practice changed from "cost plus" to fixed price contracts.

Because of the possibility that the correlation between government contract R&D and private R&D spending may be influenced spuriously by the IR&D payments to performers of the R&D contracts, IR&D was removed from the statistical relationship between government contract R&D and private R&D expenditures in the following manner: The data for IR&D originally compiled by

Reppy (1976) and Shapley (1980) were extended for later years with the data obtained from government agencies, converted to calendar years and expressed in constant dollars. These amounts of IR&D were then subtracted from private R&D spending and introduced into the equation for private R&D as a separate independent variable.

In the R&D investment equation so modified, the IR&D variable had a negative coefficient with a value near unity, but it was not statistically significant. The estimate of the effect of contract R&D on private R&D was unchanged at .27. While these results do not fully explain the economic role of IR&D, they suggest that it may be a substitute for direct private expenditures by the contracting companies; however, the basis for this inference is not firm. Yet it is clear that the positive and large relationship observed between contract R&D and private R&D spending (27 cents per dollar) is not in any way due to the practice of the overhead reimbursement of defense and space contractors for some of their R&D spending.

IV. Effects of Government R&D on Productivity Growth of the Economy

The overall effects of government R&D and of private R&D on productivity were examined at the macroeconomic level by means of a productivity function estimated for the years 1948-81 for the private business economy. No direct estimate of lags of government R&D on productivity could be obtained. Therefore, the same six-year lag was used for government R&D as used for private R&D to generate R&D capital stocks. Consistently significant and stable estimates of the effects of fixed capital per hour ($\dot{K}/L$), of unemployment (UHH), and of the ratio of the stock of private R&D capital to fixed capital (RDSTOCK/K) were obtained. For total government R&D, no significant estimate could be identified. However, when government contract R&D was separated from all other government R&D, a significant effect was identified for government contract R&D stock relative to fixed capital stock (GCRDSTOCK/K), but not for all other government R&D. The following logarithmic productivity function was estimated for output per hour (Q/L)by the generalized least squares method:

$$\ln(Q/L) = \underset{(6.14)}{2.14} + \underset{(1.99)}{2.56}\ \ln(K/L) + \underset{(3.82)}{.258}\ \ln(RDSTOCK/K)$$

$$+ \underset{(2.26)}{.065}\ \ln(GCRDSTOCK/K) - \underset{(2.14)}{.0095}UHH$$

(The t-ratios of coefficients are shown in parentheses; Rsq. = .99; D-W statistic = 1.91; the t-ratio for the residual autocorrelation coefficient = 1.37.)

These results provide statistical evidence--for the first time--of the possible existence of a direct effect of government contract R&D on private productivity, at least at the level of the economy as a whole, and should be tested in subsequent research. Perhaps an interindustry analysis could help identify the mechanism by which government contract R&D may influence the productivity growth of industries directly.

The above productivity function was used to analyze the productivity slowdown between the periods 1948-73 and 1973-81 in the context of a growth accounting analysis performed by Denison (1984). Denison's framework does not explicitly identify the effects of R&D on productivity that are included in the unexplained residual. Calculations based on the above equation indicate that changes in the growth of the stocks of private R&D capital and of government contract R&D capital could account for about one-third of this unexplained residual of -1.68. Specifically, the declines in the relative growth of the private R&D stock can explain a change of -.25 in the annual productivity growth between these two periods, a contribution similar to that obtained in an earlier analysis of the productivity slowdown (Griliches, 1980). The decline in the relative growth of the government contract R&D stock can explain a larger change of -.30 percent, even though the estimated elasticity of productivity with respect to the R&D capital ratio in the above productivity equation is four times smaller for the government R&D stock than for the private R&D stock. However, because of the very sharp decline in the rowth rate of government R&D stock, its total calculated effect on oductivity growth was greater than the effect of the relatively milder lowdown in the growth of private R&D stock. (Because these calculations were ased on simulation of the entire NPA economic growth model—see Terleckyj, oldrich and Levy, 1982—these results cannot be derived directly from the ormula.)

. Some Policy Issues in Government Funding of R&D

The effects of different government policies in private industry R&D nvestments were examined in the light of the theoretical risk and externality rguments for government support of private R&D and with reference to some of he empirical evidence bearing on policy choices.

High Risk and High Cost of R&D. One line of policy considers R&D as a enerally desirable form of investment that needs special public support. is need is based on the assumption that there is an insufficiency of vestment in R&D relative to other types of investment because the risk and st of projects exceeds the resources of individual firms. Government pport of R&D across-the-board is needed to bring about a socially optimal allocation of investment resources. This support can be provided by general tax and subsidy policies, which are equivalent to simple reductions in the cost of all R&D projects to private firms. Conduct of basic or of "infrastructure", research by the government can also be used to support private R&D. Such government sponsored R&D can expand the range of potential desirable private R&D projects. It can also improve the choices of individual projects made by private firms. All these government policies have the effect of making private R&D expenditures more productive in terms of private returns.

Large External Returns to R&D. Another line of policy involves support of specific R&D projects or classes of projects on the grounds that their external benefits are large and the prospective private returns are insufficient for the market process to generate the socially optimal amounts of investment in these projects.

Whether the market process leads to an efficient choice of R&D projects in terms of social returns depends on whether private returns are positively correlated with the external returns. If there is a strong positive correlation, then private project choices will also bring about the realization of external social benefits. Otherwise, potential social benefits will remain unrealized.

Data on private and social rates of return available for 53 cases of innovation from previous studies (Mansfield, et.al., 1977; Foster Associates, 1978; Robert R. Nathan Associates, 1978) were examined here for correlation between private and external (i.e., social less private) rates of return. No correlation was found between the two. To the extent that these cases may be typical, these results suggest that efficient private selection of innovation projects does not lead to an efficient selection of socially productive projects. This further implies that a different selection mechanism, presumably involving government selection of projects, is required to realize the external benefits.

The proper choice of specific innovation or R&D projects for government support requires that they have high external rates of return, but at the same time low private rates of return. Characteristics of such innovation projects include large total benefits (mass consumption), ease of imitation, small market shares of producers, large project costs, small unit profits, and limited market information. A two-criterion model for government choice of projects is sketched out. The choice problem for government decisionmakers is much more complex and much more difficult than the choice of R&D projects by a private firm. They have to select "social winners" that are also "private losers" while decisionmakers in the business sector need only to select "private winners." Hence, any application of a formal choice model in government requires careful prior analysis and program development.

Is There A Policy Gap? The present state of R&D policies in the United States is surveyed briefly. The general tax and subsidy support of R&D has been increased by the enactment in 1981 of the R&D marginal tax credit. Real private R&D spending has been rising rapidly since the mid-1970s. Also, the U.S. government has a number of programs that support basic research. The level of real outlays in these programs is at least being maintained. Hence, it is unlikely that a large general policy gap in support of private R&D

exists now in the United States. However, in view of the long-term escalation of consumer costs of energy and of health care, significant opportunities for productive government expenditures in support of innovation may exist in these two areas. Whether an R&D policy is more appropriate than other policies aimed at controlling social costs in these fields and, if so, how government R&D programs may be designed effectively for them remain to be determined. Historically, the success of government efforts to support technological progress of particular industries, has varied greatly with the nature of the industry and with the design of specific government programs (Nelson, 1982).

VI. Timing of the Effects of R&D on Productivity of the Economy

An Analytic Model of the R&D Embodiment Lag. The timing of the effects of R&D on productivity growth is analyzed for private R&D. Thus far, it was not possible to estimate empirically the time pattern and other attributes of the effects of government contract R&D and most of the other government R&D programs. However, aside from its importance in the economics of technical change, analysis of the timing of the productivity effects of private R&D is probably a precondition for a successful empirical analysis of the timing of productivity effects of government R&D.

An analytical model for estimating a distributed lag structure of the effects of R&D on productivity of the economy developed by the author (Terleckyj, 1984) is discussed. It is applied here to estimate the embodiment lags of R&D expenditures at the level of the U.S. business economy. Macroeconomic estimation is adopted as the first basic step. At the level of the total economy, interproduct, intercompany and interindustry effects of R&D

on productivity resulting from the diffusion of innovations in different utilizing industries are captured that would not be in estimates made at the more detailed industry or company level.

The R&D embodiment lag model was developed as a dynamic extension of the standard R&D marginal productivity model used for some time to estimate the effects of R&D on productivity in a single period. The one-period model requires the assumption of constant marginal productivity of R&D expenditures in all uses. While clearly an abstraction, this generalization has enabled researchers to obtain consistent and meaningful results. In the present extension of this analysis to multiple periods the assumption of constant marginal productivity of R&D over time is required. The multiperiod model permits estimation of the embodiment coefficients of R&D from the observed changes in total factor productivity as a function of past additions to R&D capital. In the present formulation, the depreciation rate of R&D as productive capital is assumed to be zero (empirical evidence suggests the depreciation rate of R&D has been small or zero). However, the model can be readily modified to allow positive depreciation of R&D when needed.

The productivity changes are defined as a function of past real expenditures for research and development by the following expression:

$$Q_t \frac{\Delta P_t}{P_t} = \Delta Q_t + \frac{\partial Q_t}{\partial R_t} \sum_{s=o}^{a} v_s \cdot E_{t-s}$$

Q is the value of output, P is total factor productivity, R is the stock of R&D capital, and E is the R&D expenditure during a period. The time period is indicated by the subscript t, the given embodiment lag period by s, and the total length of the lag period by a. The v's are the embodiment proportions applicable to the embodiment lag periods defined by s, and λ is the autonomous (exogenous) growth rate in total factor productivity which does not depend on R&D.

The left hand side of the above equation represents the real value of the productivity increase in period t, while the right hand side consists of a term that represents the value of the exogenous productivity change and a term that is the product of the sum of the increments of past R&D expenditures being embodied in period t multiplied by the (constant) marginal product of R&D capital.

Estimates of the R&D Embodiment Lag Structure. The embodiment lag structure was estimated with NSF data for private industry R&D expenditures beginning in 1953 and extended back to 1921 by the author, and the current total factor productivity data developed by Kendrick and Grossman (1980) at the American Productivity Center for the years 1948-81. Because the annual productivity changes are subject to considerable cyclical influences, to obtain stable estimates of the embodiment structure it is necessary to stabilize the estimates for cyclical change. This was done by the introduction of the lagged output and of unemployment variables into the estimating equation. Also, a series representing the number of computers in use in the United States was introduced. This was done because available data for fixed capital stock seriously understate the real value of computers used due to the failure of existent statistics to reflect declining computer prices.

A number of characteristics of the innovation processes were considered that determine the length and other attributes of the lag structure of R&D effects. In particular, the relationship between the diffusion of jets in civil aviation in the U.S. and the productivity changes of U.S. airlines reflecting these innovations was examined. A very strong, consistent relationship, with a two-year lag, was found between fleet conversion and productivity changes of the airline industry. The entire switchover period for this major innovation was spread over more than ten years. Other researchers have found even longer time lags for innovations in industry and in agriculture.

However, the actual period of embodiment and all other attributes of the lag function have to be approached empirically. Neither the length nor the shape of the expected lag structure can be imposed a priori for total R&D in the economy. Existence of such attributes as a single peak or a particular delay period between the R&D expenditures and the beginning of the resulting productivity improvements-- while perhaps plausible or even typical of major innovations-- does not necessarily characterize the lag function for total R&D. The only prior assumption that appears justified by existing research is that the sum of R&D effects on productivity is positive. Because of the uncertainty about the other characteristics of the lag structure, methods of statistical estimation were adopted that impose relatively moderate a priori constraints on the results. These methods include the ordinary least squares (OLS) lag structure, a fifth degree polynomial lag structure, and the lag structure estimates made by the Shiller (1973) method.

Estimates were made for lag periods of 10, 15 and 20 years and with data periods beginning in 1949, 1956 and 1960, respectively, and ending in 1981. Generally, the three statistical methods produced similar results, in that the time shapes of the moving average of the annual OLS lags, of the estimates of the polynomial lags, and of the structure of the Shiller lag coefficients were all essentially similar. The best fit was found for the fifth degree polynomial structure with a 15-year lag estimated with data for the period 1955-81. A 10-year lag period appeared too short, and the results for the 20-year lag were similar to those obtained for the 15-year lag period. The intermediate data period, 1955-81, was preferred because the data for earlier years (before 1953) involved interpolation and the estimates of the annual distributed lag structure for the longer data period would be heavily influenced by the interpolated values. On the other hand, a shorter data period would provide very few statistical degrees of freedom especially for the OLS estimates.

The best-fit lag structure had four years of delay before the beginning of productivity effects and a relatively flat shape of significant, all positive, effects extending from years -5 to -14. This structure was used to calculate the capital stock of private R&D. When introduced into the productivity equation for the U.S. private business economy, the coefficient of the R&D stock indicated highly significant contribution of R&D to productivity.

While the results obtained seem plausible at this time, further testing and verification are needed. In particular, it would be desirable to test the sensitivity of these results with respect to the choice of the index of the total factor productivity measure and with respect to the methods used to stabilize the estimates for cyclical variability.

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ECONOMIC EFFECTS OF GOVERNMENT R&D SPENDING IN THE UNITED STATES

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TABLE OF CONTENTS

Preface. v

Chapter 1: GOVERNMENT R&D PROGRAMS AND ECONOMIC ANALYSIS OF THEIR EFFECTS. 1

I. Diversity of Government R&D. 1

II. Evolution of Government R&D Programs 2

Table 1-1: Expenditure Trends in the Major R&D Programs of the U.S. Government, 1940-1980 3

III. An Overview of Research on the Economic Effects of of Government R&D. 4

Table 1-2: Performance of Federally Funded Research and Development by Sector. Selected years 1953-80. . . . 5

Table 1-3: Estimates of the Change in Private Industry R&D Expenditure Associated with $1.00 Change in Government R&D Expenditure. 9

Chapter 2: EFFECTS OF GOVERNMENT R&D ON PRIVATE R&D INVESTMENT: A Macroeconomic Analysis 19

I. Isolating the Effect of Contract R&D and of Other Government R&D on Private R&D Expenditures 20

Table 2-1: Estimates of the Equation for Private Industry R&D Expenditure Estimated with Different Specifications of Government R&D Expenditure GLS Coefficients Based on Annual Data for 1949-81. 21

II. Time Lags. 23

III. Size of the Effect of Government R&D 25

IV. Government Reimbursement of Private R&D Expenditures of Contractors 26

Table 2-2: Estimate of Reimbursed Private R&D Expenditure (IR&D) of Government Procurement Contractors, 1960-81. 30

Table 2-3: Estimates of the Equation for Private Industry R&D Expenditure Adjusted for Reimbursement of IR&D. . . . 31

Chapter 3: EFFECTS OF GOVERNMENT R&D ON PRODUCTIVITY OF THE ECONOMY . . . 35

I. A Macroeconomic Productivity Function. 35

Table 3-1: Estimates of the Equation for Output Per Hour in the U.S. Private Business Sector Made With Different Formulations of Government R&D Capital, 1949-81. 37

II. Role of R&D Capital Stocks in U.S. Productivity Growth, 1949-81. . . 40

Table 3-2: Historical Growth Rates in Output Per Hour in the U.S. Business Sector and in Explanatory Variables . . 43

Table 3-3: Actual and Estimated Growth Rates in Output Per Hour, U.S. Business Sector, 1948-1973 and 1973-1981 44

Chapter 4: SOME POLICY ISSUES IN GOVERNMENT FUNDING OF R&D. 49

I. Introduction 49

II. Effects of Policy on Private R&D Choices 50

Chart 1: Effects of R&D Subsidy and of Government Basic Research on Private R&D Investment and Rates of Return 51

Chart 2: R&D Subsidies With Different Relationships Between Private and Social Rates of Return to R&D 58

III. Empirical Evidence on the Correlation of External and Private Returns in Innovation Projects 60

Chart 3: Private and External Rates of Return on Individual Innovation Projects. 62

IV. Reasons for Divergence Between Private and Social Returns to R&D 65

V. A Model for Government Choice of R&D Projects in Support of Private Externalities 70

VI. Is There A Policy Gap? 74

VII. Summary. 79

Chapter 5: THE TIME PATTERNS OF THE EFFECTS OF INDUSTRIAL R&D ON PRODUCTIVITY OF THE U.S. PRIVATE BUSINESS ECONOMY· 83

I. Introduction 83

II. The Time Lag from R&D to Productivity. 85

III. The Nature of the R&D-Productivity Lag 90

Chart 1: Jets as Percent of All Aircraft U.S. Domestic Airlines. 93

Chart 2: Annual Changes in Percent of Fleet in the Jet Aircraft and in Total Factor Productivity, U.S. Airline Industry, 1960-1974 94

IV. Estimation of R&D--Productivity Lags 96

Table 5-1: Total Expenditure by Industry for R&D Performance, United States, 1921-80 98

Table 5-2: Estimates of a Lag Structure of R&D Effects on the Real Value of Annual Increments in Productivity of the U.S. Private Economy 102

V. Some Inferences and Questions for Further Research 103

Table 5-3: Estimated Lag Distribution of the Effects of R&D on Productivity of the Economy and on Profits of Companies Making the R&D Expenditure. 106

Chapter 6: SUMMARY 109

I. An Overview. 109

II. Government Contract R&D and Private R&D Spending in Industry . . . 110

III. Direct Effects of Government R&D on Productivity Growth. 113

IV. Specific Policy Issues Regarding Support of Private R&D. 115

V. Timing of the Effects of R&D Activities on Productivity of the Economy 118

REFERENCES 123

PREFACE

This report presents the results of research supported in whole or in part by the National Science Foundation Grant No. PRA 81-18098 "Measuring the Economic Impact of Government R&D." It is organized in five related self-contained articles presented as separate chapters. Chapter 6 contains the summary of the entire report.

Chapter 2 and a part of Chapter 3 was published as a joint article with David M. Levy in Bell Journal of Economics (Fall 1984) and Chapter 4 was published in the volume Strategic Management of Industrial R&D, edited by B. Bozeman, M. Crow and A. Link (D.C. Heath and Co., 1984).

Comments and suggestions received in preparation of these articles are acknowledged separately in the introductory notes to the individual chapters as are other sources of support provided for the work reported here. The references, consolidated for the entire report, are shown at the end.

I would like to thank Jacqueline Rupel for editorial and other assistance in preparing this report.

Nestor E. Terleckyj

August, 1984

CHAPTER 1

GOVERNMENT R&D PROGRAMS AND ECONOMIC ANALYSIS OF THEIR EFFECTS

An earlier version of this chapter was circulated as an unpublished NPA paper "Analyzing Economic Effects of Government R&D Programs," 12-8-81. I would like to thank David Levy, Albert Link and F. M. Scherer for comments and suggestions.

I. Diversity of Government R&D

It is generally well-recognized that the effect of the different government-supported R&D activities on the private economy, its productivity growth or its organizational structure varies among the government programs. Because most of these programs are quite different, across-the-board generalizations and analyses based on aggregation of data for R&D expenditures of the U.S. government require a great deal of caution.

Aggregation of private R&D outlays, has been analytically fruitful in many instances. However, expenditures for R&D by business firms are more homogenous than the government expenditures for R&D. They all represent a particular type of business investment and, subject to variations in risks and market conditions, can be analyzed generally within the same framework. More caution is needed in dealing with government R&D, because the objectives of government R&D are much more diverse than the objectives of the private R&D. Consequently, generalization of analytical results obtained in any one research study regarding the economic consequences of government R&D is much less likely to be valid than generalization of research results for private R&D.

II. Evolution of Government R&D Programs

Programs of support for research and development by the United States government were undertaken in response to a wide variety of policy concerns faced at different times in history. These programs then evolved with changes in policy needs and with the state of science and technology. Some of the government R&D programs, notably agricultural research, and research on boat safety, and some public health research had their beginning well back in the 19th century. Other programs are of quite recent origin, such as renewable energy sources research.

The major program components of government expenditure for research and development based on the budget data of the federal government agencies for the period 1940-80 are shown in Table 1-1. The largest is the expenditure for defense R&D. Other historically major components of federal R&D spending are those for the space program, for the health research programs, mainly those of the National Institutes of Health, for energy, initially for atomic energy and recently for energy research in general, and for agricultural research which has a long history of government support.

Before World War II almost all government funded R&D was performed within the federal government and in state land grant colleges. Now, the R&D expenditures of the U.S. government finance a wide range of diverse activities performed in government facilities, in industry, in private and public universities and in various research institutions.

TABLE 1-1

EXPENDITURE TRENDS IN THE MAJOR R&D PROGRAMS OF THE U.S. GOVERNMENT, 1940-1980

(millions of 1972 dollars)

Fiscal Year	Total	Defense	Atomic/ Energy
1940	255.0	90.8	0
1945	4196.0	1655.2	2265.9
1950	2021.7	1217.9	413.4
1955	5437.7	4323.1	633.5
1960	11272.5	8229.7	1435.1
1965	20022.6	9047.3	2044.1
1970	17207.0	8297.3	1767.1
1975	15571.0	7457.4	1789.3
1980	17842.0	7425.0	3255.0

(Continued)

Fiscal Year	Aeronautics and Space	Health	Agriculture	All Other
1940	7.6	9.6	100.1	46.8
1945	63.6	9.0	88.9	113.4
1950	101.8	73.9	99.0	115.8
1955	121.3	115.4	118.3	126.1
1960	583.7	471.9	191.3	360.8
1965	6849.0	992.3	276.6	813.2
1970	4104.0	1379.2	326.5	1332.9
1975	2601.6	1726.8	339.0	1657.0
1980	2819.0	1955.0	389.0	1999.0

Sources: 1940-1969: Federal Funds for Research and Development, National Science Foundation NSF70-38, Vol. XIX, Table C-88, Page 229 ff.
1970: Federal Funds for Research and Development, NSF 79-318, Vol. XXVIII, Table C-108, Page 162.
1971-1980: Federal Funds for Research and Development, NSF 80-318, Vol. XXIX, Table C-108, Page 164.
Deflated by the implicit deflator for GNP.

A substantial proportion of R&D financed by the Federal government is performed in industry, most of it under contract with the Department of Defense and NASA. This pattern of industry contracting emerged in World War II and continued with some changes since. Table 1-2 shows the government R&D expenditure by performer based on the National Science Foundation survey data beginning 1953. Earlier, according to U.S. Department of Defense data reported by Blank and Stigler (1957), industry performed R&D funded by nonindustry sources (i.e., U.S. government) was about 20 percent. In 1945, that proportion was 57 percent. Based on the data in Table 1-2, 52 percent of federal R&D funds was used in industry in 1953. This proportion rose to 59 percent in 1965, but declined subsequently to 48 percent in 1975 and 47 percent in 1980. Federal contract R&D has been declining as proportion of all R&D performed in industry for some time: from 55 percent in 1965 to 36 percent in 1975 and 32 percent in 1980.

III. An Overview of Research on the Economic Effects of Government R&D

The economic impact of government R&D outlays has been studied much less and is less well understood than the impact of the private R&D spending. The research on the effects of government R&D generally has been conducted along three lines: (1) analyses of possible effects of government-financed R&D performed in industry on productivity of the performing industries and of their customer industries; (2) effect of agricultural research on productivity of agriculture, and, most recently (3) effect of government R&D on private R&D spending. Other types of economic effects of government R&D expenditures received less research attention.

TABLE 1-2

PERFORMANCE OF FEDERALLY FUNDED RESEARCH AND DEVELOPMENT BY SECTOR. SELECTED YEARS 1953-1980
(millions of 1972 dollars)

Year	Total	Federal Government	Industry	Universities and All Other*
1953	$ 4,680	$ 1,717	$ 2,431	$ 532
1960	12,719	2,512	8,852	1,355
1965	17,498	4,159	10,408	2,930
1970	16,284	4,460	8,506	3,318
1975	14,396	4,256	6,841	3,299
1980	16,576	4,444	7,812	4,320

Sources: National Science Foundation, National Patterns of Science and Technology Resources, 1982, NSF 82-319, pp. 24, 28. The original data was deflated by the implicit GNP deflator given in Economic Report of the President, 1984.

Productivity Effects of Government R&D

A positive impact of the intensity of research and development activities (ratio of R&D outlays to output, or of R&D manpower to total employment) on productivity growth has been rather well established for the manufacturing industries for some time (Terleckyj, 1960; Minasian, 1960). After further distinctions were made between privately-financed and the government-financed R&D conducted in industry, the estimated impact of the private R&D on productivity became larger and more strongly evident, while no effect of government-financed R&D on the productivity of industries performing it could be identified (Leonard, 1971; Terleckyj, 1974). These results were confirmed in a number of subsequent studies for different industry units and periods, most recently in a rather detailed analysis of total factor productivity growth in 27 manufacturing industries over the period 1957-76, by Griliches and Lichtenberg (1984) and in a macroeconomic analysis by Levy and Terleckyj (1983) which is presented here in Chapters 2 and 3.

Similarly, though less conclusively, there were comparable differences in the estimated productivity effects of private and of government-funded industrial R&D embodied in purchased inputs. The embodied R&D was estimated from data attributing the cost of R&D conducted in the industries performing R&D through the flows in the transactions and capital input-output matrices (Terleckyj, 1974, 1982B). Frequently, positive estimates were obtained for the effects of private R&D embodied in purchased inputs, but such results for the government R&D were not stable and sometimes even negative. This is puzzling because some obvious major adaptations of the results of the defense and space R&D were made in the private economy notably in civil aviation, communication, computers, and in health care. Nevertheless, no stable statistical relationship could be estimated. A recent analysis of productivity change in commercial aviation (Lenz, Machnic and Elkins, 1981) found that the expenditures for aircraft R&D have made large contributions to airline productivity. However, in that analysis, government and privately-financed R&D expenditures were combined and the effects of the government R&D expenditures were not examined separately.

For some of the government research programs, most strikingly in agriculture, government-financed R&D was clearly and directly undertaken in order to increase private productivity. It has succeeded very well in this objective (Griliches, 1964), and a positive relation between government expenditure for agricultural research and productivity growth of American agriculture has been found over long historical periods (Evenson, 1978). Use of agricultural research expenditure by the government is now standard in the estimation of production models of the U.S. agriculture (Lu, Quance and Cline, 1979). A study of returns from agricultural R&D in different periods (Evenson, Ruttan and Waggoner, 1979) found continued high rates of return to agricultural R&D with no evidence of a decline in productivity impacts of that R&D in the 1970s.

There were also studies made of other smaller government R&D programs and their productivity impact, for example, a study by the Charles River Associates (1981), of the effects of semi-conductor technology R&D of the National Bureau of Standards on industry growth. There is no reason to think that government-financed R&D programs aimed directly at raising productivity either of particular industries or of the economy in general do not succeed in this objective. However, most of government expenditures for R&D have other principal objectives.

Effect of Government R&D on Private R&D Investment

A number of research studies completed in the past several years produced results which indicate that government R&D expenditures have, on the whole, stimulated additional private R&D expenditures. The individual studies differed greatly in methodology, data sets, levels of aggregation and time periods. Some of these studies were conducted at the microeconomic level of the individual companies. Others were done at the industry level and one was conducted at the level of total private business sector within the framework of an economic growth model. Some of these studies were cross-sectional, others used time series approach. In some cases they included attempts to estimate the lag structure of this relationship. However, they all produced estimates of a significant impact of government R&D expenditures on private R&D expenditures.

The quantitative results of these studies are summarized in Table 1-3. Excluding two extreme results, for reasons discussed below, these estimates range from 8 to 34 cents of private expenditures induced by a dollar of the federal outlays. They suggest a time lag of 3-4 years involved in this effect. (Since the completion of the project reported here more studies of the interaction of private and government R&D were completed. Table 1-3 has been partially updated in May 1984. However, these other results, which also include some estimates of negative relationship between private and government R&D cannot be presented or discussed here. They are being examined in a project currently underway.)

TABLE 1-3

ESTIMATES OF THE CHANGE IN PRIVATE INDUSTRY R&D EXPENDITURE ASSOCIATED WITH $1.00 CHANGE IN GOVERNMENT R&D EXPENDITURE

Study	Change in Private R&D	Method, Data And Period of Estimate	Time Lag Estimate
Goldberg, 1979	.02	Time series model, 14 manufacturing industries, 1958-1975.	Current year coefficient: -.03; coefficient for one year lag: .05
Levin and Reiss 1984 (Table 8.5, Cols. 2-5)	.08 to .12	Pooled data for 20 manufacturing industries, 1963, 1967 and 1972.	Not estimated
Levy and Terleckyj 1983 (Table 1)		Macroeconomic model equations, U.S. business sector, 1949-1980.	
Government R&D performed in industry	.27		Current year, only
All other government R&D	.19		Year (-3), only
Link, 1982A		Cross-section regressions 275 U.S. manufacturing companies, 1977.	
Total private	.09		Not estimated
Development	.09		Not estimated
Mansfield and Swizter, 1982, energy R&D		Survey responses, 25 companies, 1977	
Increase in government R&D	.12		2 years: 06 ea.
Decrease in government R&D	.69		3 years: .25 for 2 years; .19 in 3rd year

(continued)

Table 1-3 (continued)

Scott, 1984	.08 to .10	Cross-section regressions, for 437 companies reporting for 259 FTC lines of business categories, 1974.	Not estimated
Terleckyj, 1984B electrical eq., and communication R&D	.337	Sectoral model equation, 1948-80.	.001, .063, .087, .100 and .086 for years (0), (-1), (-2), (-3), and (-4), respectively

Sources: See References.

In the study by Goldberg (1979), the government-financed R&D outlays were introduced as a factor determining productivity of privately-financed R&D and through it indirectly the demand for private R&D investment. Working with the time series data for 14 industries for the period 1958-75, Goldberg estimated an R&D investment model. This model contained estimates of the impact of federal R&D expenditures on the private R&D expenditures lagged one year and of unlagged R&D. A longer lag structure was not considered. The sum of the two-year estimates yields a positive but low estimate of the total effect on private R&D.

A microeconomic study by Mansfield and Switzer (1982) was based on responses of 25 major firms in the R&D-intensive industries conducting 41 individual federally-funded energy R&D projects, to the question of how would an increase or a decrease in the amount of funding in the government-financed energy projects performed by these companies affect the expenditure of its own funds for R&D by the company. The responses were asymmetrical between increases and decreases in the federal funding. For increases, the respondents indicated that private R&D expenditures would be increased by .06 cents per dollar in each of the first two years after a one-time increase in federal funding. For decreases they indicated that private R&D would be reduced by 25 cents for each federal dollar in the first two years following the cut and by an additional 19 cents in the third year. Cumulated for three years these responses imply a total reduction of private R&D expenditure of 69 cents resulting from one dollar cut in government R&D spending for this program. This may reflect diminishing returns as the authors point out. However, this response may also reflect an expectation of the decline in government market for the results of private R&D implicit in the cut of government R&D support in this still very new field. In that case this particular estimate may be too high as an indicator of the magnitude of the impact of government R&D on private R&D in general.

Levin and Reiss (1984) estimated a cross-sectional model of R&D and market structure, involving concentration ratios, advertising, private R&D and government R&D, based on the data for 20 manufacturing industries. Their estimates of the equation for private research and development expenditure included in that model contain significant coefficients for government R&D. The relevant versions of this equation indicate that a dollar of federal R&D expenditure induced additional private R&D expenditure between 8 and 12 cents.

In a study of the determinants of the composition of private R&D spending of 275 companies in 1977, between basic research, applied research and development, Link (1982A) included federal R&D expenditure among the explanatory variables. He found significant relationships between federally-financed R&D performed in the company and the total company R&D spending, with a coefficient of .094, and between federally-financed R&D and company expenditure for development, with a coefficient of .086. The estimated coefficients for basic research and for applied were both negative and the coefficient for applied research was small. Neither was significant at the 5 percent level.

In a recent study of the possible effects of company expenditures for advertising and for R&D on seller concentration, Scott (1984) has examined the relationship between government-financed R&D and company-financed R&D at the level of individual lines of business within same companies. He used 3388 detailed observations for 437 companies reporting their activities in 259 lines of business to the Federal Trade Commission for 1974. For the entire group of observations Scott found significant coefficients ranging from .076 to .10 for the elasticity of private R&D spending with respect to public R&D spending. Analyzing only observations which had positive company-financed R&D Scott found significant coefficients ranging from .075 to private R&D expenditures rather than substitutes for them.

In a recent macroeconomic study by D. Levy and the author (1983), estimates of the stimulation of private R&D expenditure by government R&D spending were developed within the framework of the economic growth model of the National Planning Association (Terleckyj, Holdrich and Levy, 1982). This macroeconomic analysis explored a number of issues regarding the government R&D. These results are presented in greater detail in Chapter 2. Specifically, the study inquired whether in the economy as a whole, private spending for industrial R&D is influenced by the total government expenditure for R&D, or only by government-financed R&D performed in industry. Aggregate estimates were made indicating an effect of 27 cents per dollar for government contract R&D performed in industry and 19 cents per dollar for government R&D performed elsewhere (i.e., in government and universities and nonprofit institutions). Stronger statistical results were obtained for the government-financed R&D performed in industry.

This analysis did not succeed in clearly identifying the time lags involved in these effects of government R&D. The Levy-Terleckyj analysis did not find any lag structure for government R&D performed in industry and a possible indication of a 3-year lag for other government R&D. The

Mansfield-Switzer survey indicates similar lags involving three years. These seem to be relatively short lag periods. It could be that some of the longer time lags, e.g., those inherent in basic research, simply are too weak or too variable and could not be identified from the data. On the other hand, it has also become apparent in the Mansfield and Switzer study that companies often initiate proposals for federal R&D projects to complement R&D programs that they already carry out with their own funds. Prevalence of this pattern on a relatively large scale would tend to shorten the lag periods appreciably and may even result in negative annual lag situations reflecting private R&D spending induced by expectation of future government-financed projects. In an earlier analysis, the authors (Levy and Terleckyj, 1982) obtained indications that two separate lag structures may be involved, a shorter lag for the federally-financed R&D performed in industry, and more delayed lagged effects for government R&D performed in government, universities and in nonprofit institutions. Further analysis of those separate effects had to be deferred to a future study.

Some Inferences and Hypotheses

The fact that private R&D funds were spent to develop applications of results of government R&D to private uses may also explain the widely noted absence of a direct statistical relationship between government R&D and productivity growth in the industries conducting this R&D, and perhaps also in the industries purchasing capital and intermediate inputs from the industries conducting federal R&D. One would expect that in their allocation of the private R&D expenditures stimulated by the results of government R&D, business enterprises would invest their funds in the same manner as they would invest in a field of technology developed with private funds only, i.e., equalizing on the margin the risk-adjusted return from all types of R&D investments. Then, in case of all government R&D programs in which the purpose of government R&D spending is the support of missions of government agencies rather than support of productivity of the private sector, only the induced private R&D would have an observable effect on productivity growth.

The possibility of a direct productivity impact on the industry performers of government R&D should be examined again when the total government R&D can be separated into process and product R&D. It has been found (Link, 1982C; Scherer, 1982) that in the case of privately-financed R&D, the R&D directed towards the development of new processes does affect productivity of the performing industries, while the product R&D is more likely to affect the productivity of the customer industries. Whether the same is the case for the government R&D cannot be established at present because the data on government-financed R&D performed in industry has not been separated into product and process R&D, and separate private estimates are available only for a few industries (Scherer, 1981).

The results of previous research discussed above suggest by their cumulative evidence that government R&D stimulates private R&D. This evidence was obtained at the level of companies and industries which perform government-financed R&D and at the aggregate level of the private business sector. Significant statistical estimates of the stimulating effect on the private industry R&D, were obtained by Levy and Terleckyj for both government-financed R&D performed both in industry and for that performed in all other organizations. These positive estimates probably are best interpreted as the aggregate net effects. They are consistent with smaller indirect effects and with partial, local and short-term negative effects or crowding out.

These research results also suggest that direct stimulation of private R&D investments by the performers of government contract R&D is not the only way in which government R&D may stimulate additional private R&D expenditure. It suggests that the influence of government R&D operates across the economy from government to industry and most likely also between companies and industries.

Results of government R&D performed in a company may be transferred to other companies in the same or in other industries in a variety of ways. In many cases such transfers occur through movement of R&D personnel. Other important channels include technical and scientific journals and all sorts of written and oral communication. Some government agencies, notably the National Aeronautics and Space Administration, maintain elaborate programs to facilitate transfer of technical information and commercialization of the results of their R&D. They also adopt policies, for example with regard to patent licensing, intended to facilitate transfer of government-financed technology to private sector uses. Also, successful adaptation of government-funded results to general market products by the performers of government R&D contracts is likely to stimulate additional R&D spending by imitators.

Therefore, it is not unreasonable to expect stimulation of private R&D by government R&D to occur outside the companies performing the original government R&D. Indeed, one may postulate existence of a complex network of effects of government R&D on private R&D, the elements of which could be identified by the available research techniques. If all the estimating procedures were properly specified and the negative effects were outweighed by the positive effects, in the aggregate of all levels of organization and in the long run, one would expect the estimate of the magnitude of the direct impact of government-funded R&D to be largest at the national level because it would reflect the secondary and other induced interactions. It should be of an intermediate size at the industry level because at that level it would include only the effects of government R&D performed in the given industry, and smallest at the company level because it would exclude the intercompany effects. By how much these estimates should differ and what negative offsetting effects may be involved, and where are empirical questions. These differences may vary among programs. For example, the magnitude of intercompany and interindustry transfers may depend on the type of policies that a government agency may have with respect to facilitating transfers of R&D results from private performers and from the government. Other things being equal, one would expect these external effects of government R&D to be greater for the R&D expenditures of agencies which have active policies for facilitating such transfers. The study of the National Bureau of Standards R&D program in semiconductor technology mentioned earlier (Charles River Associates, 1981), illustrates how technology developed in a government laboratory may interact with private R&D. Specifically, in one particular analysis the contributions to company growth made by the rate of company patenting activity, were found to be significantly higher in the semiconductor companies which maintained frequent contact with the NBS laboratory conducting the semiconductor R&D than in other companies in the industry.

Estimates summarized in Table 1-3, are not inconsistent with the existence of such various cross-effects. The national level coefficient obtained by Levy-Terleckyj is considerably larger than all the others, except the estimate obtained in Mansfield-Switzer survey for the decrease in government R&D. However, these studies are too few and too diverse with regard to data, time period and methodology, to establish a pattern. Development of controlled comparable estimates for different levels of the economy will require considerable research efforts in the future.

CHAPTER 2

EFFECTS OF GOVERNMENT R&D ON PRIVATE R&D INVESTMENT:
A Macroeconomic Analysis

David M. Levy and Nestor E. Terleckyj

This Chapter and the first section of Chapter 3 are drawn from the article "Effects of Government R&D on Private R&D Investment and Productivity: a Macroeconomic Analysis", by David M. Levy and Nestor E. Terleckyj, Bell Journal of Economics Vol. 14, No. 2, Autumn 1983, pp. 551-561. In addition it includes in Table 2-2 extended constant dollar estimates of the reimbursed private R&D expenditures of government contractors (IR&D). The authors would like to thank Lincoln Anderson, Ray Boddy, Zvi Griliches, Martin Holdrich, Albert Link, Edwin Mansfield, David Moore, M. Ishaq Nadiri, J. R. Norsworthy, F. M. Scherer, John T. Scott, and two anonymous referees of The Bell Journal of Economics for many helpful comments and suggestions. The authors alone are responsible for any remaining errors. Research reported here was supported by the National Science Foundation grant PRA-81-18098. "Measuring the Economic Impact of Government R&D", and by the National Planning Association work in the development of the NPA economic growth model. Partial suport at an earlier stage of this research was provided to Terleckyj by the Alfred P. Sloan Foundation and to Levy by the Sabre Foundation.

In this chapter, the role of government R&D expenditures in influencing private R&D spending is first examined at the macroeconomic level. Hypotheses regarding the presence, magnitude and timing of the effects of government-financed R&D performed in industry, called here "contract R&D" and of other government funded R&D are tested first. Then, the possible influence of the overhead allowance for private R&D expenditures of government contractors on the observed relationship between government contract R&D and private R&D outlays is considered. It has been the practice of the Department of Defense (DoD) and the National Aeronautics and Space Administration (NASA) to allow such reimbursements in order to maintain general technological capabilities of these companies.

The strategy of research followed here provides for verifying the results by variation of the period of analysis and employment of alternative estimating techniques, including robust regression techniques. Because the quality of R&D data for earlier years of the 1949-81 period for which most estimates are made may not be as good as for the later years, the critical estimates are replicated for shorter periods in order to provide a sensitivity test for the results at the cost of a much reduced number of observations.

The effects of government R&D expenditures on private R&D investment and on productivity are estimated as regression coefficients in the respective equations for private industry R&D expenditure and for productivity of the U.S. business sector. Generally, the results reported here are based on generalized least squares (GLS) estimates. These estimates are replicated by autoregressive least squares procedures and autoregressive least absolute errors (LAE) procedures. Where important differences in the results are obtained they are noted.

I. Isolating the Effect of Contract R&D and of Other Government R&D on Private R&D Expenditures

The basic approach to the analysis of the impact of government R&D on private R&D spending is by estimating the investment function for private R&D expenditure in which government R&D is a determining variable. A series of estimates of the R&D investment equation, involving different specifications of the government R&D variables estimated by a GLS method for the period 1949-81, are shown in Table 2-1.

In these equations, all dollar amounts are in billions of 1972 dollars.

TABLE 2-1

ESTIMATES OF THE EQUATION FOR PRIVATE INDUSTRY R&D EXPENDITURE ESTIMATED WITH DIFFERENT SPECIFICATIONS OF GOVERNMENT R&D EXPENDITURE GLS Coefficients Based on Annual Data for 1949-81 With Money Amounts in Billions of 1972 Dollars (t-ratios in parentheses)

Variable	(2.1-1)	(2.1-2)	(2.1-3)	(2.1-4)
Constant	-1.59 (2.14)	-2.29 (3.03)	-2.23 (3.50)	-1.26 (1.53)
Output, Q(-1)	.0095 (4.09)	.012 (4.95)	.012 (6.76)	.0080 (2.71)
Corptax(-1)	- .018 (1.21)	- .026 (1.77)	- .025 (1.88)	- .0093 (.59)
RDAGE. RDSTOCK(0)	.0021 (3.93)	.0019 (3.94)	.0019 (4.19)	.0024 (4.63)
Unemployment (0)	- .114 (1.84)	- .139 (2.23)	- .136 (2.28)	- .123 (2.11)
GRD (0)	.208 (6.97)			
GCRD (0)		.277 (6.18)	.273 (8.56)	.266 (8.86)
GORD (0)		- .018 (.15)		
GORD (-3)				.187 (1.76)
Serial Correlation Coeff. rho*	.54	.41	.42	.40
Durbin-Watson Statistic	1.86	1.87	1.88	1.94
Remaining Serial correlation, rho	.03 (1.05)	.03 (.95)	.03 (.94)	-.01 (.20)

*Computed by a maximum likelihood iterative procedure.

The private industry expenditure for R&D performance in any given year depends, aside from government-financed R&D, on the value of output of the private business sector (Q) lagged one year on corporate taxes (CORPTAX) also lagged one year, on unemployment, in millions (UHH), and on the product of the average age of the R&D stock in years (RDAGE), and the stock of private industry R&D capital (RDSTOCK). The stock of R&D capital was compounded without depreciation but with a six-year embodiment lag from R&D expenditure. The basis of this procedure is discussed by Terleckyj (1982A).

Different formulations of government R&D were explored because the effects of different types of government R&D are likely to vary. Thus, the contract R&D performed in industry may have an identifiable relationship to private R&D outlays, quite distinct in magnitude or timing from the effect of government-funded R&D performed elsewhere, i.e., in universities, government facilities and nonprofit institutions.

In Equation 2-1.1, the total federally-funded R&D performance (GRD) is included. A highly significant coefficient is obtained, suggesting an induced private R&D expenditure of 21 cents per dollar of government R&D expenditure. However, when the total government expenditure for R&D performance is separated into contract R&D performed in industry (GCRD) and all other government R&D (GORD), estimates of two very different effects are obtained in Equation 2.1-2. A larger regression coefficient of .27 is obtained for contract R&D, while the coefficient for all other federally-funded performance is practically zero (small, negative and without any statistical significance). Dropping the variable for other federal R&D in Equation 2.1-3 does not change the estimates of the effects but it strengthens the significance of the remaining coefficients.

II. Time Lags

There are good a priori reasons to expect that the private R&D expenditures made to adapt the results of of federally-funded R&D to commercial products follow the performance of government-funded R&D with some time lag. This should apply to contract R&D performed in industry and especially to other federal R&D which includes large components of basic research funded by the National Science Foundation (NSF) and the health research programs of the National Institutes of Health (NIH). Furthermore, the more basic components of federal R&D performed outside industry can be expected to involve longer lags than the applied research and development projects typical of contract R&D.

No significant estimates of the lag structure of contract R&D were obtained. The estimates that were made, implied short lags represented by coefficients which were not statistically significant. The estimated mean lags were not statistically different from zero. An alternate estimate of Equation 2.1-3 was made by the OLS method with an autoregressive term for the dependent variable, which was generally consistent with Equation 2.1-3. The autoregressive coefficient of .36, when interpreted as a geometric lag coefficient, also implies a very short lag structure with 64 percent of the impact occurring within the same year as the contract R&D. This absence of a significant lag structure for the contract R&D is somewhat surprising. One possible explanation may be that the performing companies have learned to form realistic expectations about future government support by developing R&D proposals for the government in a way which takes into account their plans for future complementary R&D funded with their own resources. Mansfield and Switzer (1982) found that the companies performing government R&D contracts which participate in the formulation of the projects to be funded by the government are more likely to derive spin-off benefits from these projects than the companies which do not. Similarly, companies which have learned to plan their R&D programs effectively are more likely to have learned to integrate these programs well in advance with the projects they perform under contract for the government.

For other federal R&D, an indication was obtained that there may exist an average three-year lag in its relationship to private R&D spending. The coefficients for that year stood out as the strongest among alternative lags. Hence, the other R&D variable (GORD) is lagged 3 years in Equation 2.1-4. A rather large coefficient was obtained for that variable indicating an effect of 19 cents induced in private industry R&D expenditures per dollar of federal outlay for R&D performed outside industry after a three year lag. A restriction that this coefficient is statistically the same as the .27 coefficient estimated for contract R&D could not be rejected by the F-test at any reasonable level of significance F(1,26) = .48. However, the coefficient for other government R&D is only significant at the 10 percent level.

Estimates of Equations (2.1-2) and (2.1-4) that were made for the shorter period 1959-81, and are not shown here, suggested a possibly larger impact of the federal expenditures for R&D performed outside of industry than the equations included in Table 2-1. These aggregate estimates, however, were not stable. Also, no estimates of a distributed lag structure could be obtained for either contract or other government R&D. Clearly, further analysis is needed of the timing of the impact of both contract R&D and other government-funded R&D on private R&D expenditure. Effects of other government-funded R&D should be investigated at a more disaggregated level by type of program, by agency and separately for basic research, applied research and development.

III. Size of the Effect of Government R&D

Estimates of the effect of government contract R&D on private R&D spending obtained here are considerably higher than estimates obtained in the other studies mentioned earlier. However, to the extent that the government contract R&D stimulate private R&D expenditures in companies and industries other than those performing the given R&D contracts, the estimates of the net effect of government R&D made at the macroeconomic level should be expected to be larger than the estimates made at lower levels of aggregation which would not capture these cross-effects. To the extent that there also appear to exist positive effects of government-funded R&D performed outside industry, i.e., of the government-supported basic research, of agricultural research, intramural DoD research, etc., additional interorganizational effects might be identified that could not be observed by intraorganizational regressions. The ability to capture such intersectoral and interorganizational effects is one of the advantages of macroeconomic analysis which compensates for some of its disadvantages resulting from its reliance on very high levels of data aggregation and limited number of statistical observations.

One consistent result of the present analysis is that the relationship between the amounts of contract R&D and the level of private R&D expenditure is both strong and stable. The estimated magnitude of the effect of contract R&D on private R&D remains consistently at about .27 and is statistically significant in the different equations that were estimated with different variables, for different periods and by different methods. Before accepting these statistical results as clear evidence in support of the hypothesis that, in the aggregate, government contract R&D stimulates private R&D outlays, one further test needs to be performed. In particular, it is important to ascertain whether, and to what degree, the observed strong statistical relationship between contract R&D performed in industry and private industry expenditure for R&D may reflect the practice of government agencies to reimburse government contractors for their own R&D expenditure as part of the contract overhead.* To a considerable degree, the firms performing R&D under contract for the government and the firms selling goods and services (including R&D services) to DoD and NASA, to which this overhead reimbursement applies, have been the same.

IV. Government Reimbursement of Private R&D Expenditures of Contractors

In the early 1960s DoD and NASA began allowing an overhead factor for R&D expenditures of their procurement contractors, including the R&D contractors, as an indirect cost of goods sold to the government. Little analytic attention has been paid to this practice and the amounts involved were not generally known to the economists and statisticians concerned with R&D activities until fairly recently. Judith V. Reppy (1976) analyzed the different aspects of the program and published some aggregated data as well as her analyses of unpublished company data that were made available to her. Subsequently Willis H. Shapley (1980) completed a study of the subject for the NSF and made estimates of the amounts involved for the period 1964-79. This R&D, also called "independent research and development" (IR&D) is different in concept and content from R&D performed by companies for the government directly under an R&D contract. The principal objective of IR&D is to strenghthen the technology base of the defense and space industries by allowing the contractors independent research initiatives without tight contractual or administrative controls. Shapley points out, however, that a clear dividing line between the reimbursed overhead R&D and contract R&D, and even privately-funded R&D, cannot be drawn because there is considerable overlap in the types of projects performed by the contractor companies in the three categories of funding.

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* We are indebted to Edwin Mansfield and to F. M. Scherer for pointing out this possibility.

The contract R&D data, as reported by industry performers in the NSF surveys, has been used in estimating the R&D investment equations reported in the previous section. The overhead allowances for private R&D spending are viewed as private expenditure by the companies receiving it and are so reported in the R&D industry survey data base of the NSF. The amounts of IR&D if reimbursed under R&D contracts are treated as government R&D outlays in the federal funds data set of the NSF, but reimbursements to procurement contractors providing goods or services other than R&D to DoD are not included as government R&D outlays in that data set.

While IR&D is reported in the industry survey data as private R&D expenditures, nevertheless, from the point of view of investment behavior of the firms, the government overhead payment represents cost sharing in the company's R&D program by the government. It may displace some of the company own spending if the remaining opportunities are not sufficiently attractive to induce the firm to extend its R&D spending by an amount equal to that of the government reimbursements. According to Reppy and Shapley, the contractors are allowed considerable latitude regarding what type of R&D they may charge in the overhead. Hence, the reimbursed R&D may simply be viewed by the companies as a replacement for what they would fund themselves. This substitution has a dynamic aspect. Reppy points out that IR&D funds provide an organizational cushion which permits continuity of R&D staffing and projects. Under this arrangement the funding and the accounting for these activities reflected in the statistics may be a function of changes in the flow of R&D contracts.

The government reimbursement of R&D is then equivalent to a reduction in the cost of private R&D which leads the companies to move down their investment schedule. The effect of the overhead reimbursement on private R&D outlays then, may be negative, neutral, or positive, depending on the elasticities of the R&D investment schedules of the companies involved over the relevant segments. On the other hand, the effect of contract R&D may be to stimulate additional private R&D outlays needed to commercialize the results of R&D done for the government.

Regardless of the possible differences in the effects on private R&D expenditure of contract R&D and of reimbursed R&D, the fact that the R&D contractors and the firms receiving the overhead reimbursement are largely the same companies, raises the possibility that the observed correlation between contract R&D and private R&D expenditure is simply a statistical appearance resulting from this overlap and does not reflect any real stimulating effect from contract to private R&D.

To test the hypotheses that the effects of contract R&D and of reimbursed R&D are distinct, and that the estimated effect of contract R&D is independent of the overhead reimbursement, the previously estimated R&D investment equations are respecified to include IR&D as a separate variable. Also, the private R&D series is adjusted to exclude overhead reimbursed R&D.

In order to perform these tests with the best information available, Shapley's series were extended by the data obtained from DoD and NASA for 1980 and 1981. Also, we have deflated the series by the GNP deflator, converted it to calendar years, and extrapolated it back to include positive amounts for 1961, 1962 and 1963 to minimize the abrupt transition from the period before any overhead allowances for private R&D were granted and the first year for which Shapley obtained the information. The original fiscal year data and the constant dollar calendar year series derived from them are shown in Table 2-2. (The need to convert the data from fiscal to calendar years needs to be confirmed in the future.)

The equations for private R&D expenditure adjusted to exclude IR&D but with IR&D introduced as a separate variable are reported in Table 2-3. The first three equations report estimates for the period 1949-81. The first equation (2.3-1) repeats for the adjusted dependent variable, the estimates of the Equation (2.1-3) from Table 2-1 which was discussed before. The effect of federal contract R&D is now estimated at 25.3 cents instead of 27.3 in the previous equation. The new regression coefficient is still highly

TABLE 2-2

ESTIMATE OF REIMBURSED PRIVATE R&D EXPENDITURE (IR&D) OF GOVERNMENT PROCUREMENT CONTRACTORS, 1960-81

(millions)

Year	Fiscal Years, Current Dollars	Calendar Year 1972 Dollars
1960	—	$ 0
1961	—	89
1962	—	229
1963	—	370
1964	320	451
1965	335	484
1966	384	529
1967	427	568
1968	471	603
1969	508	569
1970	480	534
1971	495	483
1972	432	456
1973	479	462
1974	496	451
1975	541	448
1976	585	465
1977	644	469
1978	692	473
1979	763	485
1980	869	521
1981	1089	597

Sources: Fiscal years, current dollars series: 1963-79, Shapley (1981), p. 10; 1980-81 DoD, NASA. These estimates were converted first to calendar years by weighting fiscal year data by the number of quarters applicable to the given year and then deflated to 1972 dollars by the implicit GNP deflator. To minimize the abrupt transition at the beginning of the period the calendar year data were extrapolated by the values shown.

TABLE 2-3

ESTIMATES OF THE EQUATION FOR PRIVATE INDUSTRY R&D EXPENDITURE
ADJUSTED FOR REIMBURSEMENT OF IR&D
Based on Money Amounts in Billions of 1972 Dollars
(t-ratios in parentheses)

	1949-81			1959-81	
	(2.3-1)	(2.3-2)	(2.3-3)	(2.3-4)	(2.3-5)
Variable	GLS	GLS	IAE	GLS	IAE
Constant	-1.72 (2.49)	-2.20 (3.07)	- .507	.671 (.26)	- .497
Output, Q(-1)	.010 (5.19)	.012 (5.70)	.0056	.0078 (1.98)	.0054
Corptax (-1)	- .019 (1.34)	- .025 (1.72)	- .0077	- .014 (.54)	- .011
RAGE. (0) RDSTOCK(0)	.0021 (4.17)	.0019 (4.04)	.0018	.0027 (3.88)	.0017
Unemployment (0)	- .112 (1.85)	- .135 (2.14)	- .296	- .187 (2.38)	- .285
GCRD (0)	.253 (6.85)	.272 (7.57)	.194	.140 (.89)	.200
IR&D (0)		- .955 (1.40)	-1.36	.177 (.15)	-1.34
RDINDADJ(-1)			.460		.489
Serial Correlation Coefficient, rho*	.52	.42		.62	
Durbin-Watson Statistic	1.93	1.88		1.56	
Remaining Serial Correlation, rho	.01 (.03)	.03 (.89)	- .24 (7.40)	.22 (4.44)	.05 (1.09)

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* Computed by a maximum likelihood iterative procedure.

significant. In Equation (2.3-2), the overhead reimbursement is introduced explicitly as a separate variable. The coefficient for reimbursed R&D is very large and negative, -.955, but it is not statistically significant. More important, the coefficient for the contract R&D is raised back to .272 and its statistical significance is strengthened. Some of the other coefficients in the equation are similarly affected. These results are consistent with the hypothesis that the observed effect of contract R&D represents a real effect on private R&D and is not the result of overhead reimbursement of government contractors. The estimated effect of reimbursed R&D is not significantly different from zero. However, the two effects are different. The restriction that the coefficients of contract R&D and IR&D are the same is rejected by an F-test at the 10 percent level of significance F(1,26) = 3.09.

These estimates could not be replicated directly for the shorter period 1959-81. In the GLS equation (2.3-4), both coefficients for government R&D are not significant. However, the distribution of residuals of that equation is highly skewed. An estimate of this equation for both periods was made by the LAE method which is not as sensitive to outlying observations as the least squares methods. The resulting coefficients obtained in Equations (2.3-3) and (2.3-5) are very similar for both periods and are consistent in magnitude and sign with the GLS estimates for the period 1949-81 in Equation (2.3-2).

The results obtained here for government contract R&D are rather robust. They are consistent with the hypothesis that the contract R&D induces within a fairly short time additional private R&D expenditures in the range of 20-30 cents per dollar of contract R&D, with the statistically strongest estimates made for coefficients of about 27 cents.

On the other hand, no consistent or significant estimates of the effects of IR&D were obtained. There are strong hints that the net effect of IR&D on private R&D outlays is large and negative, but the estimates obtained are unstable. Additional evidence is needed, preferably from analysis at the company level, before conclusions about this effect can be drawn with any confidence. Nevertheless, in view of these results, it may be advisable in future analyses of private industry R&D investment behavior, especially in analyses involving government contractors, to perform at least some sensitivity tests using R&D expenditures adjusted to exclude the government-reimbursed R&D and perhaps introducing the amounts of IR&D explicitly as a separate variable. The unadjusted data may produce biased results.

CHAPTER 3

EFFECTS OF GOVERNMENT R&D ON PRODUCTIVITY OF THE ECONOMY

I. A Macroeconomic Productivity Function

by David M. Levy and Nestor E. Terleckyj

(See the Introductory note to Chapter 2)

The possibility of the existence of direct effects of government R&D on productivity is examined here by estimating a productivity equation which includes, among others, a series of variables based on differently specified stocks of government R&D capital. The productivity equation used here expresses the logarithm of the output per hour in the U.S. private business sector, in 1972 dollars, as a linear function of the logarithm of the stock of fixed capital per hour (also in 1972 dollars), unemployment (in thousands), the logarithm of the ratio of stock of private industry R&D capital to fixed capital and the logarithm of the ratio of government R&D capital to fixed capital.

Government expenditures for R&D are compounded here into R&D capital stocks by the same method as the private R&D stock, i.e., assuming a six-year embodiment lag and no depreciation, as was discussed in Chapter 2. No direct estimates of the embodiment lag for government R&D are available. The government R&D data used is the NSF series for the cost of government-funded R&D performance which begins in 1953. This series has been extended back by linking it to the data in an earlier DoD series with a few additional adaptations.

A series of estimates of the productivity equation made by GLS method for

1949-81 with different concepts of government R&D capital, are shown in Table 3-1. In Equation (3.1-1) separate ratio variables were introduced for the stocks of private industry and government R&D. Very different estimates of the effects of these two ratios were obtained. Private R&D-fixed capital stock ratio has a relatively large coefficients, .267, which is highly significant statistically, but the ratio with the total government R&D stock has a relatively small and statistically insignificant coefficient in this equation. The restriction that the private and total government R&D have the same coefficient in Equation (3.1-1) was rejected, decisively, by an F-test at the 2 percent level.

Next, the government R&D was divided into contract R&D and all other government R&D in Equation (3.1-2). Government contract R&D in this equation has a positive and significant effect which, however, is smaller than the effect of private R&D stock. Other government R&D, on the other hand, apparently has no significant relationship to private-sector productivity. Its coefficient is negative, but not significant. The restriction that the coefficients for contract R&D stock and the stock of other government R&D in Equation (3.1-2) are the same is rejected statistically at the 10 percent level. F(1,27)=3.05. Only government contract R&D is included along with private R&D in the next Equation (3.1-3). The restriction that the positive effect of contract R&D is the same as the larger positive effect of private R&D stock in Equation (3.1-3) was also rejected by the F-test, at the 3 percent level, F(1,28)=5.61. Recomputing the stock of other government R&D using a longer embodiment lag of nine years instead of six, produces a negative and significant coefficient for other government R&D capital in Equation (3.1-4). However, this result may be not meaningful because

TABLE 3-1

ESTIMATES OF THE EQUATION FOR OUTPUT PER HOUR IN THE U.S.
PRIVATE BUSINESS SECTOR MADE WITH DIFFERENT FORMULATIONS OF
GOVERNMENT R&D CAPITAL, 1949-81

GLS Regression Coefficients in Equation for Ln (Q/L)
(t-ratios are in parentheses)

Variable	3-1.1	3-1.2	3-1.3	3-1.4*
Constant	1.99 (6.00)	2.08 (6.51)	2.14 (6.14)	2.06 (7.43)
Unemployment: UHH	- .013 (3.09)	0 .0088 (1.92)	0 .0095 (2.14)	0 .012 (2.69)
Fixed Capital Per Hour: ln(K/L)	.322 (2.66)	.264 (2.20)	.256 (1.99)	.308 (2.84)
Company R&D ln(RDSTOCK/K)	.267 (3.96)	.268 (4.19)	.258 (3.82)	.314 (5.03)
Total Government R&D: ln(GRDC/K)	.050 (1.63)			
Government Contract R&D: ln(GCRDC/K)		.094 (2.13)	.065 (2.26)	.057 (1.42)
Other Government R&D: ln(GORDC/K)		- .047 (1.14)		
Other Government R&D 9-year lag: ln(GORDC9/K)				- .031 (2.41)
Serial Correlation Coefficient, rho*	.51	.47	.55	.39
Durbin-Watson Statistic	1.88	1.92	1.91	1.78
Remaining Serial Correlation, rho	.06 (1.85)	.04 (1.13)	.05 (1.37)	.11 (3.23)

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* Data available only for 1950-81. Computed by a maximum likelihood iterative procedure.

substantial multicollinearity is present.

The effect of government contract R&D on private sector productivity which is observed here may represent either the direct effect on the productivity of performing companies or the indirect effects of producer goods developed as spillovers of government contract R&D on productivity of the users of these products. These effects cannot be distinguished at the aggregate level. However, a recent study of manufacturing industries (Terleckyj, 1982B) suggests government contract R&D had a positive impact on the productivity of industries purchasing inputs from the industries performing this R&D but not on the performing industries themselves. Hence, the productivity impact observed by the macroeconomic analysis is likely to reflect the technology transfer to the industries using the spillover results of the government-funded contract R&D from the performers of contract R&D.

The lack of correlation of the stock of government funded R&D other than the contract R&D with productivity is somewhat surprising considering that it includes the government expenditure for agricultural research which is usually found to be clearly correlated with productivity of the agricultural sector (Evenson, 1978; Lu, Cline and Quance, 1979) as well as the basic research programs of NSF and other agencies. Using this variable with longer time lags even produces a negative estimate of its effect. Clearly, these relationship require further examination. Most likely, at the aggregate level, the other government R&D capital cannot adequately represent the relationships that exist between private-sector productivity and government R&D programs at such lower levels of aggregation. Also, the distinctions between basic research, effects of both private R&D (Mansfield, 1980) and government contract R&D

(Link, 1981) need to be examined. These and other possibilities, however, cannot be explored further here, and their analysis is deferred to future research.

To summarize this analysis, stable and statistically significant estimates were obtained of a relatively large effect of private R&D capital on private sector productivity, with elasticity of about .28. Also, a significant relationship was found for the first time between the stock of government contract R&D and productivity. However, the magnitude of this effect is much smaller than that of the private R&D, with estimated elasticity of about .065. (Because the magnitudes of the two R&D stocks have been quite similar in most years of the period, the elasticity estimates give a good approximation of the relative effects on productivity of absolute changes in the two stocks.) This indicates that spillover effects of government R&D on private sector productivity do occur and that they are significant, but that for the most part the government contract research has been mission-oriented and did not generate private sector benefits comparable to the effects of direct private investment in R&D. The estimates of the effects of contract R&D were obtained by applying the embodiment lags estimated for private R&D in compounding government R&D capital. In the future, an attempt will be made to obtain direct estimates of the embodiment lags for government contract R&D. possible.

No significant positive effect of all other government R&D on private sector productivity was found. Again, the tentative conclusion is that analysis of the effects of other government-funded R&D probably requires disaggregation of this total into more homogeneous components which may have rather different effects on the private sector productivity. Unlike in the case of private R&D, there is no reason to expect that the marginal products of government R&D in private sector production would tend to be equalized among the different government R&D programs.

II. Role of R&D Capital Stocks in U.S. Productivity Growth, 1949-81

This analysis was originally prepared for discussion at the Conference on "International Comparisons of Productivity and Causes of the Slowdown" at the American Enterprise Institute for Public Policy Research, September 30, 1982, Washington, D.C.

The question of the effect of changes in R&D capital stocks on the U.S. economic growth is examined here in the context of the growth accounting analysis provided by Denison (1984). It appears that about one-third of the slowdown in Denison's unexplained residual ("advances in knowledge and miscellaneous determinants") may be explained by changes in the stocks of industrial R&D. This conclusion is based on the contribution of private and government funded industrial R&D capital to productivity growth as estimated above.

In the productivity equation (Equation 3.1-3 from Table 3-1) the logarithm of the output per hour in 1972 dollars (Q/H) depends linearly on the logarithm of the net fixed capital stock per hour (K/H), also in 1972 dollars, on the logarithms of the ratios of the private and government R&D capital stocks to the fixed capital stock (R/K) and (GR/K), respectively, and on the level of unemployment (UHH), in millions. The equation coefficients and their t-ratios (in parentheses) were estimated by a generalized least squares method as follows:

$$\ln(Q/H) = \underset{(6.14)}{2.14} + \underset{(1.99)}{.256} \ln(K/H) + \underset{(3.82)}{.258} \ln(R/K)$$

$$+ \underset{(2.26)}{.065} \ln(GR/K) - \underset{(2.14)}{.0095}\ \mathrm{UHH}$$

(D-W = 1.907; t-ratio for the residual autocorrelation coefficient, rho, = 1.37; R-SQ. corr. = .99)

In estimating the stocks of R&D capital, the annual real R&D expenditures were compounded into the two respective R&D capital stocks using zero rate of depreciation for R&D and a six-year embodiment lag of R&D expenditure into R&D capital used in production. This procedure is based on results of empirical research which have been described elsewhere (Terleckyj, 1982A). The R&D capital stocks are introduced in the equation as the numerators of their ratios to the fixed capital stock in an attempt to allow for the joint impact of R&D and fixed capital on productivity. In this approach the R&D stocks enhance productivity of fixed capital in the same way as the fixed capital as introduced in the capital-labor ratio enhances productivity of manhours.

All regression coefficients estimated in this equation for the period 1948-81 are statistically significant at least at the five percent level. However, the elasticity coefficient for the ratio of the private industry R&D stock to fixed capital is almost 4 times larger than the coefficient for the ratio of stock of government contract R&D performed in industry, even though the two R&D stocks are of comparable size. This is not surprising. The main purpose of the government contract R&D has been to support government defense and space programs and not private productivity. Private sector spillovers are largely incidental to the main objectives of this R&D, though they turn out to be significant. Also, additional private R&D expenditures are usually required to embody the results of government R&D in products developed for the private sector and the contribution of this R&D to national productivity is reflected in the coefficient of the ratio of private industry R&D capital to fixed capital.

The estimated productivity equation can be used to analyze changes in productivity growth that have occurred in the United States and specifically to search for the developments that have contributed to the productivity slowdown after 1973. Table 3-2 shows for the two periods analyzed by Denison, the growth rates in the variables included in the above productivity equation.

Alternate changes in productivity were calculated from the equation for the periods 1948-1973 and 1973-1981 by varying the values of the independent variables to estimate the contribution of actual changes in these variables to productivity growth in the two periods and to its slowdown between these periods. The results are shown in Table 3-3.

Out of the total slowdown in productivity growth experienced between these two periods of 2.22 percent (close to Denison's total change in national income per person employed of 2.60 when adjusted for hours per worker) the variables included in the equation together explain 1.01 percent. The unexplained residual is 1.21.

The estimated contribution of the slowdown in growth of fixed capital per hour is -.44, considerably higher than Denison's -.12. The estimated effect of changes in unemployment, is somewhat smaller here, -.02, but

TABLE 3-2

HISTORICAL GROWTH RATES IN OUTPUT PER HOUR IN THE U.S. BUSINESS SECTOR AND IN EXPLANATORY VARIABLES

(annual percentage changes)

	1948-73	1973-81	Difference 1973-81 Less 1948-73
Output per Hour	2.94%	.72%	-2.22%
Fixed Capital-Labor Ratio	3.57	1.85	-1.72
Private R&D-Fixed Capital Ratio	4.70	3.42	-1.28
Government R&D-Fixed Capital Ratio	17.05	1.79	-15.26
Unemployment	2.64	8.32	5.68

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TABLE 3-3

ACTUAL AND ESTIMATED GROWTH RATES IN OUTPUT PER HOUR, U.S. BUSINESS SECTOR, 1948-1973 AND 1973-1981

(annual percentage changes)

	1948-73	1973-81	Difference: 1973-81 Less 1948-73
Actual Growth Rate	2.94%	.73%	-2.22%
Growth Rate Components Estimated by the Productivity Equation:			
Total	2.52	1.51	-1.01
Fixed Capital-Labor Ratio	.91	.47	- .44
Private R&D-Fixed Capital Ratio	1.20	.95	- .25
Government R&D-Fixed Capital Ratio	.43	.13	- .30
Unemployment	- .02	- .04	- .02
Difference: Actual Less Estimated	.42	- .79	-1.21

comparable to -.04 listed by Denison under "intensity of demand."

The slowdown in the growth of the two R&D stocks relative to the growth in the fixed capital stock accounts together for -.55 of the slowdown. These factors appear implicitly in Denison's analysis as part of the residual "advances in knowledge and miscellaneous determinants". Slowdown of the growth in the two R&D stocks could thus account for one third of the residual growth slowdown estimated by Denison.

The relative slowdown in the private R&D stock accounts here for -.25 percent of productivity slowdown, very similar in magnitude to an earlier estimate by Griliches (1980). However, in the present equation, the government R&D, for the first time, also has an estimated significant direct effect on private sector productivity. This is in addition to any indirect effect it may have had by influencing the growth of private R&D spending, through the relationship discussed in Chapter 2 and which of course appears here as the effect of private R&D.

While the estimate of a significant productivity effect of government funded R&D is not as strong as in the case of private R&D investment, at least it does not appear to depend on the formulation of the econometric model. Similar significant effects of government R&D capital have been found also in alternate formulations of the macroeconomic production and productivity models of the private business sector. This effect probably represents spillover effects from the industries performing government R&D to industries purchasing capital goods from the performing industries (Terleckyj, 1982B). Direct effects of this R&D on productivity of the performing industries could not be identified, but they were apparent in productivity changes of the total business sector which includes both performing and purchasing industries.

Because there has been a large absolute decline in real levels of government contract R&D spending in the late 1960s and early 1970's, while the real private R&D continued to grow, though at a reduced rate, the growth rate of the ratio of the stock of government R&D to fixed capital declined much more than did the growth of the ratio of private R&D stock to fixed capital. This is shown in Table 3-2. Consequently, the estimated contribution of the decline in the stock of government contract R&D relative to the fixed capital stock to the slowdown in productivity is even larger than the contribution of the slowdown in the growth of private R&D stock.

The slowdown in the growth of R&D stocks (and presumably in the associated other complementary investments in innovation) relative to fixed capital thus may explain together -.55 or roughly one fourth of the slowdown in productivity growth. While this is not a decisive component in explaining the productivity slowdown, the combined effect of changes in the two R&D stocks which have not been measured before, represents a notable factor in the change in the growth rate of the economy.

We are still left with a large amount of uncertainty about the causes of productivity slowdown. However, assuming that the present estimates are correct, we may begin to make quantitative assessments of the components of productivity change attributable to changes in the stocks of R&D. The main question for future research at this stage concerns the independence of the individual determinants of productivity. In particular, this question involves the degree to which R&D stocks (and presumably stocks of other technological investments too) interact with the fixed capital and also with human capital in actually effecting change in productivity.

CHAPTER 4

SOME POLICY ISSUES IN GOVERNMENT FUNDING OF R&D

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I. INTRODUCTION

This paper considers a model of choice of R&D projects by the private industry and the impact of the different government policies -- general subsidies, complementary research, regulation and direct support of specific projects by the government -- on the choice of innovation projects conducted in the nation and on the private and social benefits these projects produce for the economy. The empirical evidence regarding the existence of a correlation between the private and the external benefits of completed innovation projects is examined. The paper discusses the criteria for a government R&D policy of project choice and describes a possible model for government project selection aimed at optimizing the social returns to R&D. Finally, a brief overview of current R&D policies in the United States is given and the possibility of the existence, at present, of major policy gaps is assessed.

The present discussion deals with the analytic aspects of policy issues concerned with private sector optimization. It does not consider policy issues involving R&D performed as an input in government production of public goods, such as national defense. Also, it is recognized that important institutional arrangements for policy implementation clearly exist, but they are not discussed in this paper.

Originally, this paper considered the economic rationale for government support of R&D projects in the private sector which could potentially result in substantial savings of energy but which were not attractive to private investors in R&D. Energy is clearly an intermediate good for consumers as well as business. Energy savings resulting from improvements in economic efficiency--i.e., lower costs of identical goods and services as distinct from reductions of the quantity or quality of real consumption—represent an important and unambiguous case of improvement in economic well being in the United States at present. Hence, many of the case illustrations or potential applications discussed here apply to energy saving R&D projects.

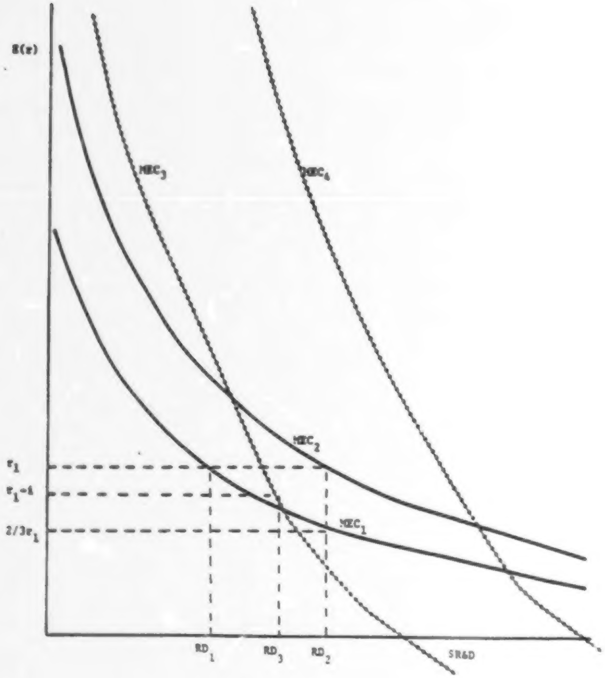
II. EFFECTS OF POLICY ON PRIVATE R&D CHOICES

Private R&D Investment Schedule

The process of project choice for private research and development expenditure may be represented by a graphic description of the opportunities for profitable R&D investment perceived by a firm at a given time. These opportunities can be depicted by a schedule of marginal efficiency of capital, ex ante as perceived by the prospective investors in R&D, representing by a continuous function the expected rates of return on investments in the available discrete R&D projects, and their separate component tasks (subject to decision at the same level), arranged in the order of diminishing rate of return. This schedule is shown as MEC1 in Chart 1. Actual realization of these expected returns is subject to considerable

CHART 1

EFFECTS OF R&D SUBSIDY AND OF GOVERNMENT BASIC RESEARCH ON PRIVATE R&D INVESTMENT AND RATES OF RETURN



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uncertainty and risks. The empirical evidence discussed in the next section strongly suggests that: (1) the selection of R&D projects by private industry is imperfect, but (2) the selection is not random and is generally successful on the average.

Business firms undertake R&D investments only to the point where the expected rate of return on the known available projects includes an adequate premium for risk. This point is shown as r1. The relatively high private rates of return to private R&D investment that have been estimated consistently on the average, in case study analyses (Mansfield, et. al., 1977; Foster Associates, 1978; Robert Nathan Associates, 1978), are an indication that such risk premiums are in fact being realized. One apparent reason for existence of these premiums is the high rate of failure of the innovation projects that were actually implemented. These case studies also found that about 30 percent of innovations carried out to completion failed economically.

Reasons for Government Intervention: Risk and Externalities

Existence of relatively high risks and the presence of externalities have been viewed as proper reasons for government intervention in the R&D investment process for some time. Because R&D investment is more risky than other investments, and even the large firms cannot fully selfinsure against these risks, there may be underinvestment in R&D. The private resources invested in R&D are not invested to the point where social returns from all types of investments are equalized on the margin. The government should then reduce the private cost of R&D by a subsidy or advantageous tax treatment of R&D and bring about a better allocation of resources in the economy.

Another well known case for intervention is that the benefits from R&D may not be appropriable by the private investors. Hence, the incentive to invest is insufficient and the government should subsidize or fund R&D outright (Arrow, 1962), especially where the external benefits are large. One type of R&D projects for which the inappropriability of benefits has been particularly well recognized is basic research (Nelson, 1959).

For the most part the earlier formulations of policy analyses for R&D assumed that information produced by R&D flows freely and is in effect a public good. More recently the imperfections, cost and difficulty of transmitting information have been recognized as an obstacle in achieving desirable technological change (Mowery, 1983). In fact, the U.S. government has had programs to help transfer new information and technology to producers for quite some time. The classic case of this is the agricultural extension service. The extension service was found to interact positively with agricultural R&D (Evenson, 1978) raising the social rate of return to R&D and possibly to total social investment in innovation.

Subsidies to Compensate for Private Risk in R&D

One policy approach is to overcome the inadequacy of investment in R&D by reducing the risk to private investors. This can be done by reducing the private cost of all R&D across the board. Another policy approach which is discussed in the next section, is for the government to try improving the private project selection process so that the *ex ante* decision schedule moves closer to the actual *ex post* realization schedule of returns by reducing the project selection errors.

A general reduction of the cost of R&D can be implemented through general subsidy, such as the 25 percent tax credit on increases in R&D that was enacted in 1981, or by similar generally applicable measures. The effect of such general incentives for companies to undertake more R&D is illustrated analytically in Chart 1 by a 1/3 tax credit for total R&D which (disregarding here possible difference in timing, in tax liability, and in relative variance of risks, as well as the practical implementation problems, that may occur in reality) has the effect of raising the private rates of return on total R&D investment (including government share of cost) by 50 percent. The MEC1 curve shifts to MEC2, and the total R&D investment rises from RD1 to RD2 of which the government now pays 1/3 RD2. Another way of portraying the same result is to specify that the required expected return is reduced to 2/3r1. Since by definition expected return along the MEC1 = 2/3 MEC2 the same result is obtained. By this policy the government may choose the level of total R&D investment which will equalize, the rate of return to R&D with the returns to other investments.

Other government activities, especially those in R&D contracting for defense and other government programs and in the field of education also can affect the cost of private R&D in industry. Performance of R&D contracts for the government may lower the cost of private R&D, e.g., through training or development of products applicable to private markets. Link (1982) found some statistical evidence of such an effect. Specific aspects of such effects are not considered in this paper.

Policies to Reduce Private Risk and Uncertainty in R&D

Another way in which government can help companies to improve their average returns on private R&D investment, and under some conditions raise the level of private R&D investment as well, is to conduct basic or system or other infrastructure--"generic" or "infratechnology" R&D (Tassey, 1982). Perhaps the classic and successful case of the public policy of support of a modern high technology industry in the United States, was the conduct of the R&D in aircraft development by the National Advisory Council on Aeronautics before World War II which benefitted firms in the industry in general by advancing the generally applicable technical knowledge (Mowery and Rosenberg, 1982). Such R&D serves companies in an industry across-the-board by producing either basic research results or other knowledge which helps companies to avoid unprofitable investments by reducing uncertainties in project selection. The choice of projects by companies is probabilistic and hence reduction of uncertainties would reduce risk to investors in choosing among the same available projects.

Such a government program also reduces the required risk premium, say by i to r1-i. Because the investment schedule shifts to the right, this may, but not necessarily would, raise the amount of private R&D investment. This is shown in Chart 1 by a shift from MEC1 to MEC3. The MEC3 containing the same projects has necessarily a steeper slope because due to better project selection, it is closer to the true ex-post ranking of projects by their realized rates of return. In the present illustration, the private R&D expenditure increases from RD1 to RD3. The marginal return is lower, but the average return realized ex post (area under the MEC3 divided by RD3 compared to the area under MEC1 divided by RD1) may be higher because some losses are avoided. The cost of government-financed R&D is not shown in the diagram.

The effect of such facilitating R&D funded by the government is likely to extend beyond improving the private selection among a fixed set of projects. Two further consequences of such government-funded R&D projects are likely, both of which would tend to increase the amount of private R&D performed: (1) they may create new technological opportunities for R&D projects throughout the range of returns, in effect enlarging the content of MEC3 and adding additional investment possibilities at its various points creating a new schedule shown by the MEC4, and (2) they may reduce the cost of prospective projects and the required return r1 with the result analogous to that produced by a subsidy but with a better investment choice schedule. Whether the amount spent by the private firm for R&D increases or decreases depends on the reduction in the risk premium, i, resulting from improved predictability of outcome and on the size of the increase in investment opportunities, i.e., the extent of shift from MEC3 to MEC4 in the relevant region.

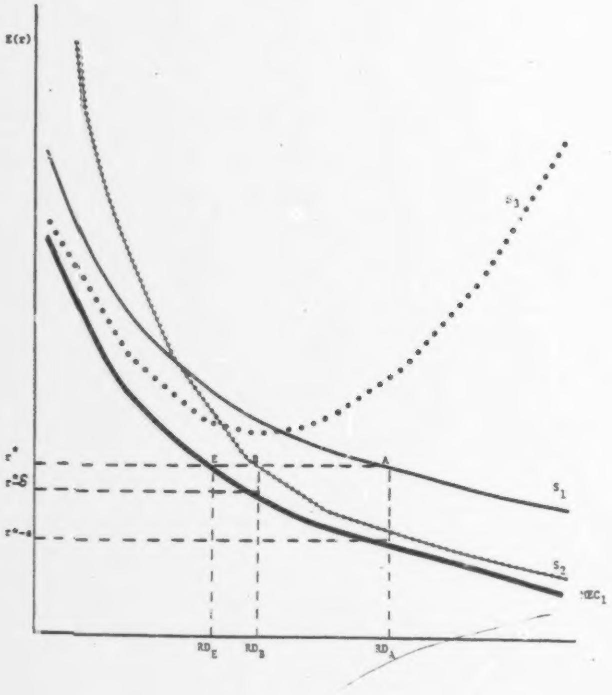
Except for the government selection of the infratechnology projects, such indirect methods of support of private R&D by the conduct government R&D projects leave the R&D project choices and the level of R&D investment to the business sector as does a general subsidy. However, government selection of individual R&D projects for public funding is more likely than a general subsidy to have differential effects on the success of R&D projects conducted by the individual companies with their own funds.

Policies to Realize External Benefits from R&D

With regard to externality the argument is quite different. All that is shown in Chart 1 are the private returns to the company (companies) on their R&D investments. For any given marginal efficiency of capital schedule there may exist externalities, i.e., economic returns to R&D not realized by the private R&D investors, which either are or are not correlated with the private returns shown in the MEC schedules. If the presence of externalities in the available projects is completely uncorrelated with the size of private returns, then the expected external returns are constant throughout, and the curve, S1 in Chart 2 representing the expected social rate of return under these conditions is vertically parallel to MEC1. In this situation there is not much the government can do by general measures, such as tax credits, other than to increase the subsidy, e.g., by the amount s, and to push the investment down the social investment schedule S1 in Chart 2, beyond the point where private risk is compensated, shown by E, to the point where the marginal social return to R&D is in line with, r*, the marginal social returns from other investments. (The rate of return r* in Chart 2 corresponds to 2/3r, in Chart 1. The subsidy required to realize external benefits is independent of and additional to the subsidy required to compensate for additional risk in R&D.) This is depicted by point A, in Chart 2. The proportion of subsidy required to move the private investment from

CHART 2

R&D SUBSIDIES WITH DIFFERENT RELATIONSHIPS BETWEEN PRIVATE AND SOCIAL RATES OF RETURN TO R&D



point E to point A is s/r*.

If the correlation between private and external return is strong and positive as shown by S2, there is comparatively little scope for government involvement other than by general risk alleviation measures already reflected in point E. Considering the uncertainties involved, point B may be too close to E to warrant a subsidy at the rate of δ and, more importantly, government involvement, which is likely to have large fixed costs of its own and possibly other risks as well.

On the other hand, if there exists a systematic negative correlation between private and external return, then Schedule S3 of Chart 2 applies. The projects with low private but high social return constitute a natural object for government funding. However, a direct search for such projects would have to be developed because indirect incentives to industry would not realize these externalities. Private ordering of priorities yields perverse externality results. Moreover, at the negligible levels of expected private returns where large externalities are present in S3, private incentives which are essential for a successful general subsidy program would break down.

It is important to distinguish between these three possible situations because their policy implications are quite different. In the first case, general stimulation of private R&D expenditures is indicated and possibly also selection of individual projects for government funding if practical. In the second case, little or no direct government involvement in project selection is appropriate. In the third, direct selection of R&D projects for government funding is necessary if optimal external returns are to be realized. Which situation applies to the projects available in the economy or in a particular field remains to be determined by examination of the relevant completed projects. An analysis of the actual patterns of correlation between private and external returns from the available case studies of private innovations may provide a starting point.

3

III. EMPIRICAL EVIDENCE ON THE CORRELATION OF EXTERNAL AND PRIVATE RETURNS IN INNOVATION PROJECTS

Studies that have been made of completed innovation projects ex-post provide some information about the attributes of projects which may be appropriate for government support, e.g., with regard to their orientation to industrial processes, industrial products, or consumer products, capital or nondurable goods and the levels of their private rates of return.

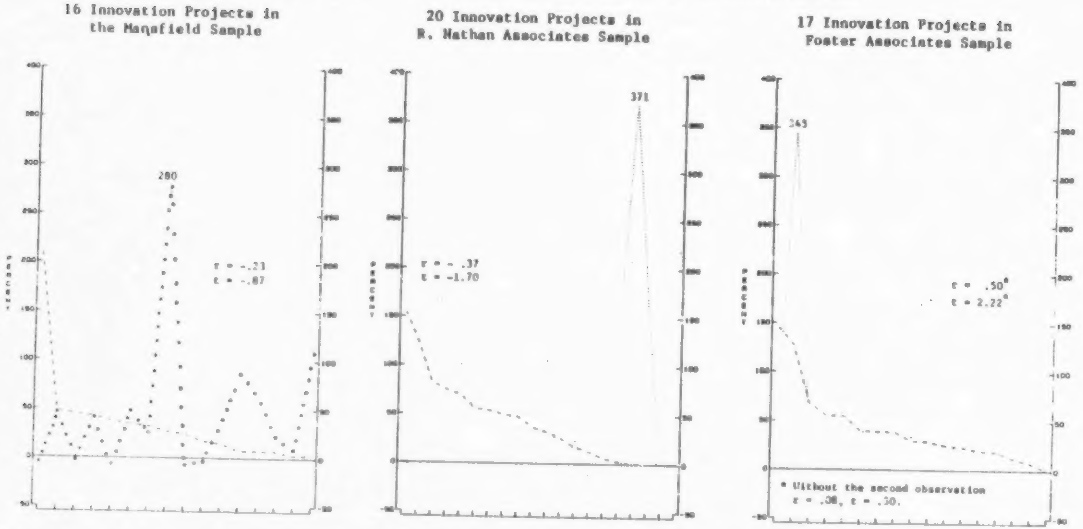
These case studies represent the results of research that was undertaken to ascertain the private and social rates of return from private expenditures for innovation including the cost of R&D and of innovation outlays. The original work by Mansfield in the article published by him and his associates (Mansfield, et. al., 1977) revealed that both private and social rates of return to private R&D have been quite high. The National Science Foundation funded two additional studies in order to test the important results obtained by Mansfield by replicating his concepts and methods with the data for two additional samples of cases. Both of these later studies, one conducted by Foster Associates, Inc., and the other by Robert Nathan Asociates, Inc., confirmed Mansfield's findings. Together these studies examined a total of over 50 innovations and computed internal private and social rates of return from these innovations, including rates of returns based on alternate sets of assumptions. The three panels of Chart 3, show the private and the external rates of return estimated for the projects included in the three case analyses arranged in the descending order of the estimated private internal rates of return.

Statistical analysis of the data in these charts indicates that there is no significant correlation between the private and the external rates of return to these innovations. There is, to be sure, a correlation between their private and their social rates of return, but this correlation is obtained simply because the private return is a substantial component of the total social return. From the policy point of view it is only the correlation between the external returns and private returns that is of interest.

No significant correlation was found between private and external rate of return when all the observations were grouped together. Nor was it found for separate groups of producer good innovations, industrial processes innovation and consumer good innovations. Private rates of return proved to be not significant in explaining the external rates of returns in the aggregate sample comprising all these cases and for the Mansfield and the R. Nathan Associates samples as is shown in Chart 3. The significant coefficient obtained for the Foster Associates sample results from a single extreme observation as is noted in the third panel of Chart 3.

CHART 3

PRIVATE AND EXTERNAL RATES OF RETURN ON INDIVIDUAL INNOVATION PROJECTS



16 Innovation Projects in the Mansfield Sample

20 Innovation Projects in R. Nathan Associates Sample

17 Innovation Projects in Foster Associates Sample

Note: External rates of return are calculated as the difference between the social and the private rates of return estimated in the case studies. Negative private rates of return are plotted at zero.

Sources: Mansfield, et. al., "Social and Private Rates of Return from Industrial Innovation", Quarterly Journal of Economics, 91 (May 1977): 211-240; Robert R. Nathan Associates, Inc., Net Rates of Return on Innovations, Vols. I, II and Executive Summary. Report submitted to the National Science Foundation, October 1978; Foster Associates, Inc., A Survey on Net Rates of Return on Innovations, Vol.s I-III. Prepared for the National Science Foundation, 1978.

A multiple regression analysis was conducted relating the external rates of return of those innovations to their private rates of return and to dummy variables characterizing these innovations as producer goods, industrial processes and consumer goods. Regressions were estimated for the three samples of innovations separately and for all observations taken together. In each case, the coefficients for producer good innovations were higher than for the other two types of innovations. For all observations grouped together, the coefficient for the dummy variable representing producer good innovations indicating an additional rate of return of 68 percent was statistically significant at the 5 percent level. However, while capital goods R&D expenditures were found in earlier research (Terleckyj 1974, Scherer 1982) to have larger relative impact on productivity of industries than other producer goods R&D, no such effect was found in this sample of innovations when capital goods innovations were separated from other producer good innovations.

It is important to stress that these case studies were conducted primarily to measure private and social returns to private investment in innovations rather than for the purpose of identifying R&D projects for government support. These cases represent the actually chosen and completed private innovations. Hence, these samples of innovations are inherently biased against projects which had low expected private returns but may have had high expected external returns. On the whole, business companies do make successful selections of projects and obtain substantial private returns.

Also, the arrangement of returns to innovations in the three panels of Chart 3 is illustrative. It does not represent the descending order in which these projects were chosen. Indeed, these projects were chosen in different fields by different companies and they include innovation projects which would not have been chosen had their actual return been known beforehand.

Nevertheless, these results are suggestive. Regarding the question of whether the case S2 prevails, i.e., whether high positive correlation exists between private and external returns, the evidence of these case studies suggests that it does not. There is no evidence of positive correlation, and there may even be a negative correlation between private and external returns. In either of these two eventualities, there is a presumptive basis for direct government support of particular R&D projects.

In order for the government support to be efficient it is necessary to identify directly characteristics of projects which qualify them for government support, i.e., projects with both low expected private returns and high expected social returns, i.e., with high external returns. While, generally, little is known about the factors which may help identify large external payoff potential, some empirical indications are available. Other characteristics of such projects can be discussed more speculatively.

IV. REASONS FOR DIVERGENCE BETWEEN PRIVATE AND SOCIAL RETURNS TO R&D

Large Social Benefits

There are empirical indications that the size of social benefits is related to the level of external returns. In analyzing the cases reported in his study, Mansfield (Mansfield et. al., 1977) found that the total size of the social benefits, in dollars, three years after the innovation was significantly related to the difference between total social return and private return. This confirmed earlier theoretical hypothesis by Arrow (1962) that the major innovations are more likely to contain large proportions of external returns than the minor ones. Mansfield's findings for the size of total return was confirmed in the study by Robert Nathan Associates. These results strengthen the case for selection for government support of R&D programs and projects aimed at innovations with large payoff prospects and especially programs aimed at important and large categories of consumer expenditures (such as food, energy and health care) rather than individual projects of moderate market potential.

R&D Would Benefit All Companies in the Industry

It is of no advantage to an individual producer in an industry to undertake basic, generic or infrastructure research which will improve productivity of R&D performed by all companies but would have either small or uncertain differential impact on the performance of the innovator. Such is the case with most of the basic research performed by industry, except to some degree by large technologically diversified companies (Nelson, 1959), and with the "infratechnology" research in general (Tassey, 1982; Mowery and Rosenberg, 1982). U.S.

government has a number of programs for support of basic research, and, to a much lesser degree, for some types of infrastructure R&D.

Small Market Shares

If organization of the industry is fragemented (as in farming, construction, or the health services) development of innovations, as well as their diffusion, is likely to require government support to realize their potential benefits to the economy. Individual firms are too small and account for too small fractions of the market to permit development of innovations with their own resources for their own benefit. These innovations have to come from the government or from large companies supplying materials or equipment to the industry, perhaps with cooperative arrangements with or without government participation.

High Probability of Successful Imitation

Another variable for which some empirical evidence is available is the cost of imitation. Mansfield (Mansfield, et. al., 1977) found that the absolute dollar amount of the cost of imitation was negatively related to the external returns. Evidently high cost of imitation acted as a deterrent to imitation and thus enabled the innovators to capture more of the social return. The extent of the risk that a company may not realize profits on an innovation it develops, depends on the probability that the innovation will be successfully imitated by other companies. Other research reported by Mansfield (Mansfield, 1984), also revealed that the cost of legally and successfully imitating even patented innovations and the time required for it have been on the average much less than was previously thought and much less than the legal life of patents, in fact only a few years.

Evidently, the technical complexity of the innovation, high cost of R&D or capital investment required for innovating in the given field and the advantage of technological or marketing lead position of the original innovating company provide the best protection of its innovation investment. On the other hand, if an innovation can be imitated by a large number of competitors within a short time, even if the market potential of the innovation is large, individual companies may find it not to their advantage to undertake the original R&D and other innovation investments. Government support of such projects may be indicated in some circumstances.

Small Unit Profits

Even if the number of companies in an industry is not very large (but more than very few), small unit profit determined by the price and market expectations in the prospective product may deter R&D investment even by large companies. An innovation with a national market potential but expected small unit profits would require realization of a very large volume of sales. However, the market uncertainty may not justify investment of private resources of a company with even a sizable regional market or a significant fraction of the national market. For example, if, hypothetically, the expected present value of benefits of the prospective innovation averages $20 for every household in the nation, the potential benefit would be over $1.6 billion. If the cost of developing and marketing the innovation is estimated at $20 million, the social benefit-cost ratio is a very attractive 80. However, if the expected value of the unit profit per household is $2, even a company with 20 percent of the national market, which is typical of many industries, would have to consider whether the uncertain potential profit of $32 million later justifies a risky investment of $20 million sooner. Considering the uncertainties of sales, of unit profit, of the actual cost of innovation and of the payout period, many such companies would probably not undertake this R&D.

Large Cost Relative to Company Resources

When the cost of an R&D project is large relative to the resources of the company that can be committed to risky investment, the project is not likely to be supported by this firm. In an industry characterized by small companies the individual firms are also likely to have small market shares, and most R&D projects may be prohibitively expensive.

But even in industries with relatively large companies the cost of potential R&D projects may be very high. For example, before World War II military aircraft was developed by the private companies and then demonstrated competitively to the Army Air Corps, and, as noted earlier, the government support was confined to conduct of supportive infratechnology R&D. However, during and after World War II, development of the more sophisticated aircraft exceeded resources of individual manufacturers, and the U.S. Government undertook financing of research and development of the military aircraft (Hitch, 1962; Mowery and Rosenberg, 1982).

At present, development of innovations in large computers is very expensive. Hence, in a number of countries development of such computers is undertaken by pooled resources of manufacturers and governments of these countries. Recently, cooperative R&D was initiated in the United States by several computer manufacturers, but without government participation. Thus, the relevant size of companies, their profitability, the industry structure and the nature of the project jointly determine whether or not the expected cost of a particular prospective R&D project is large relative to the resources of the company.

Limited Market Information or Uncertain Acceptance

The actual performance of an already developed new product may be not known to its potential users and its diffusion may be slow or halted. In case of consumers or small business users this could be due to the technical nature of information or consumer skepticism about producer claims. In case of an industrial innovation, potential users may want to wait for accumulation of experience in actual operating conditions, for subsequent improvements in the product or for more information about the product, e.g., about its training requirements. Development of such experience or information (including experience data about the human factors or environmental cost) for innovations promising large potential social benefits may justify government support of these projects to speed up diffusion of innovations. Such projects are likely to involve straightforward development work rather than advanced research. In that they deal with market information, programatically they shade over into "extension service" modes similar to that in effect for U.S. agricultural research programs. Industrial extension services are now provided on a fairly modest scale by a number of States.

V. A MODEL FOR GOVERNMENT CHOICE OF R&D PROJECTS IN SUPPORT OF PRIVATE EXTERNALITIES

The preceding discussion contains some suggestions for constructing a formal model for government choice of industrial R&D projects. A choice model for government-supported R&D projects has to reflect the objectives of government policy for support of industrial R&D. The government objective is to support projects with high *public good* content, i.e., projects with large social returns potential, but which would not be undertaken by business because of low expected returns due to the high cost, ease of imitation, market uncertainties and other attributes of high risk. This is a very challenging task.

The information problems are likely to be much more serious for the government decision makers than for those in the private sector. For example, interview methods posing the question whether or not a project was successful is very likely to produce more biased results in a government environment because of the influence of political and bureaucratic factors on the responses. Therefore, explicit and objective estimation of the social and private rates of return to prospective projects are needed for government choice of projects.

The two principal attributes of R&D projects which potentially may qualify for government support are the high expected social rate of return and at the same time low expected private rate of return. Both attributes constitute necessary conditions for government support of these projects. Thus, given

$$E(e) = E(s) - E(r) \qquad (1)$$

where the three variables are the expected external, social and private rates of return, the conditions for government choice of projects are

$$E(e) \geq e^{*} \qquad (2)$$

$$E(r) < r^{*} \qquad (3)$$

This is clearly a much more complicated decision problem than that of project selection by a private firm. The government choice makers have to ascertain not only that the project is a "winner" from a social point of view but also that it is a "loser" in the private sector. By contrast the job of the private decision makers is to pick winners for private investment:

$$E(r) \geq r^{*} \qquad (4)$$

Expected private rate of return is a function of the probability of success of the project and the expected time streams of costs and profits. Models for analysis of private R&D choices have been provided. A probability of success model was developed and estimated for private industry R&D projects by Mansfield and Wagner (1975), in which the overall probability of success is a joint result of the probability of success at each of the three stages (technical completion, commercialization, and economic payoff).

In general, the expected private rate of return on the project is given by a function such as:

$$E(ri)=f(si/S, p, I, RD, Ki, T, Ci) \qquad (5)$$

where the expected profits of company i, E(ri), depend presumably positively on such factors as the market share in the relevant product line of the company proposing the project, (si/S), the financial resources of the company (Ki), the unit profit, (p), and the cost of imitation I, and negatively on the cost of the R&D project, (RD), other costs of innovation to the company, (Ci), and the expected period of market diffusion of the innovation, (T).

Social rates of return are a function of total expected economic benefits. These benefits depend on the market potential, e.g., as defined by the expected number of users (households, firms, employees, vehicles, housing units, square feet of office space, etc.) times the average benefit per user, for example:

$$E(s) = fb(bhH, beE, RD, C) \qquad (6)$$

where bh represents average consumer benefits per household, H the number of households, be is the average employer benefit per worker, E the number of workers, RD the cost of the proposed project, and C all other costs of this innovation to the economy. Given the time paths of costs and benefits the internal social rate of return can be calculated from the time series data of (6).

A double criterion model of government choice, based on relations

(2) and (3), also needs to consider the probability of other companies than the company proposing the project for government support (or, in case of government originated proposals, of any company) undertaking this project. This can be accommodated by estimating equation (5) for other relevant companies by substituting the market share and the financial resources variables applicable to these companies. This task can be simplified by efficient search procedures and estimations of specified cut-off points for rates of return. Joint use of estimates of equations (5) and (6) should then provide a sufficient analytic basis for selection of projects consistent with the program budget and other policy requirements. Also calculation of indicators of the organizational structure of the given industry, such as the Herfindahl-Hirschman index of market shares computed for the companies which perform or could perform the R&D in the given product field may help in judging the probability that a private company would fund the given project.

These analytic requirements suggest a very large burden on public decision makers and a need for well designed and well tested program implementation.

Many of the difficulties of implementing successful policies to promote technical change result from the lack of sufficient specialized scientific and technical knowledge on the part of government officials in charge of the program. These issues have been reviewed recently in a series of studies of government influence on technical change in a number of different industries and programs (Nelson, 1982). These studies were aimed at identifying the characteristics of successful and the unsuccessful attempts by the U.S. government to promote technical change.

In addition, there are further problems of costs and imperfections of information flow and "market" imperfections of decision making in the public sector. Government programs in effect have very large fixed costs. There is a general tendency for government programs to continue and to grow over long periods of time once they acquire their own clientele and momentum of support.

However, the implementation issues encountered in development of government programs in general and also special issues concerning programs in support of R&D policies cannot be discussed here.

VI. IS THERE A POLICY GAP?

General Subsidy

The U.S. government has been encouraging private investment in industrial R&D by allowing current year write-off of all R&D expenditures. By enacting the R&D tax credit in 1981, the government took another significant step in subsidizing private R&D investment across-the-board. Whether this policy compensates enough, not enough, or too much for the inability of the market system to optimize the allocation of total investment in the economy when the investment in the more risky projects is insufficient to equalize the social marginal returns ex post is not known. This is a subject for empirical research. But it could be that no major policy gap exists with regard to general underinvestment in R&D due to its risky nature.

In fact, real private R&D investment has grown very rapidly in recent years. It did not show any of the slump that characterized the fixed investment in the late 1970s. Throughout the period 1976-1980, including the recession years 1980-1982, real (adjusted by GNP deflator) private expenditure for industrial R&D grew at about 5 percent or more each year in contrast to slower growth in the early 1970s which was interrupted by absolute declines in the 1971 and 1975 recessions:

1971	-1.3%	1977	4.9%
1972	3.9	1978	6.4
1973	7.3	1979	7.0
1974	2.9	1980	8.0
1975	-2.7	1981 est.	6.6
1976	6.3	1982 est.	5.0

The environment for private R&D investments in the early 1980s is characterized by increased recognition by corporate boards of importance of long-term goals and specifically importance of long-term investments in research and development. The foreign competition, in particular, has awakened most corporate managements in high technology companies to the need of maintaining a long-term technological competitive position of their companies. Emergence of intense foreign R&D competition has changed materially the structure of market risks in many product fields, and failure to innovate is now apparently viewed as much more risky than expenditure of funds for R&D compared to what it was considered to be ten years ago.

Basic and Facilitating Research

The U.S. government also has existing programs for project support in other socially desirable categories for which the market mechanism does not provide sufficient investment: (1) basic research; (2) agricultural research; and (3) specialized infrastructure research complementing ongoing industry-supported R&D on the industry-wide basis, e.g., the R&D projects conducted by the National Bureau of Standards in support of the semiconductor industry (Charles River Associates, Inc., 1981).

Potential Externalities in Particular Industries

A government program involving project selection for consumer or producer benefits across-the-board over diverse fields of products and markets is not feasible. For that reason no general policy gap can be identified consisting of all product fields in which externalities may exist.

However, an argument probably can be made that R&D policy gaps exist in production of health care (as distinct from basic biomedical research) and in energy conservation, because insufficient investment is made in innovation in these fields. In both of these cases the consumer outlays are large and are growing, and the market structure of producers may not provide enough incentives for cost reducing innovations. Also, substantial third party (insurance and government) payments are involved in health care, and insufficient consumer knowledge may exist in energy conservation. In both fields substantial other policy concerns are present, important foreign policy considerations in case of energy and significant fiscal and budgetary issues in case of health care. Hence, it may be desirable to explore the feasibility and appropriateness of specific policies in support of innovations in these two fields. However, before launching such programs it would be necessary to ensure that government regulation or purchasing practices already provide incentives for innovations rather than inhibit them and that an active policy in support of innovations is still needed in these fields.

Changes in Laws and Regulation

The present analysis deals with policy issues involved in government funding of industrial R&D projects. Regulatory policies with their many complexities and ramifications cannot be discussed here. However, it is important to realize that changes in the law or in regulatory practices may constitute effective alternatives to government expenditure for R&D support.

Recently, changes in anti-trust, patent and other laws have been proposed with the intent to stimulate private innovation. Of particular note is the pending legislation to change the U.S. anti-trust law by relaxing the statutory anti-trust definitions and eliminating the statutory punitive treble damage liability in private suits resulting from joint R&D ventures by companies. The pooling of risks by companies in undertaking costly and risky innovation projects jointly, may thus be achieved without government participation in funding of this R&D.

As a practical matter, such changes in policy may require a considerable time for implementation. A legislative act and an executive policy based on it may be insufficient to reassure the companies about the immunity of cooperative innovative activities from subsequent anti-trust prosecution. A series of court decisions including decisions by the Supreme Court will probably be required to establish the legality and the limits on permissible cooperative practices.

VII. SUMMARY

This paper analyzes the effect of government policies undertaken in support of private industry R&D. Two categories of such policies are considered in detail.

One line of policy involves support of R&D in general as a desirable form of investment. This support is undertaken on the assumption that there is an insufficiency of investment in R&D relative to other types of investment because the risk and cost of projects exceeds the resources of individual firms. Government support of R&D across-the-board is needed to bring about a socially optimal allocation of investment resources. This support can be provided by general tax and subsidy policies. Such measures are equivalent to simple reductions in the cost of all R&D projects to private firms. Basic, and other supportive research conducted by the government can also be used to support private R&D. This government sponsored R&D can expand the range of potential private R&D projects and can also improve the choices of individual projects made by the private firms.

The second type of policies involves support of specific R&D projects or classes of projects on the grounds that the external benefits of these projects are large and the prospective private returns are insufficient to generate the socially optimal amount of investment in these projects by the market process. The effect of an efficient market choice of R&D projects on social returns depends on whether private returns are positively correlated with the external returns. If there is a strong positive correlation, then private project choices will also bring about the realization of external social benefits. Otherwise, potential social benefits will remain unrealized.

Data on private and social rates of return available for 53 cases of innovation were examined for correlation between private and external (i.e., social less private), rates of return. No correlation was found between the two. To the extent that these cases may be typical, these results suggest that efficient private selection of innovation projects does not lead to an efficient selection of socially productive projects. This further implies that a different selection mechanism presumably one involving government selection of projects is required to realize the external benefits.

The proper choice of specific innovation or R&D projects for government support requires that they have high external rates of return but at the same time low private rates of return. Characteristics of innovation projects which may produce large social benefits and at the same time are not attractive for private investors include large total benefits (mass consumption), ease of imitation, small market shares of producers, large project costs, small unit profits and limited market information. A two-criterion model for government choice of projects is sketched out. The choice problem for government decision makers is much more complex and much more difficult than the choice of R&D projects by a private firm. Hence, any application of such a model would require careful prior analysis and program development.

The present state of R&D policies in the United States is surveyed briefly. The general tax and subsidy support has been recently increased by the enactment of the 1981 R&D marginal tax credit. Real private R&D

spending has been rising rapidly for a number of years. Also, the U.S. government has a number of projects for support of basic research, and the level of real outlays in these programs is at least being maintained. Hence, it is unlikely that a large general policy gap in support of R&D exists now in the United States. However, in view of the long-term escalation of consumer costs of energy and of health care, it may be that significant opportunities for productive government expenditures in support of innovation may exist in these two areas. However, whether an R&D policy is more appropriate than other policies aimed at controlling social costs in these fields and, if so, how government R&D programs may be designed effectively for them remains to be determined.

CHAPTER 5

THE TIME PATTERN OF THE EFFECTS OF INDUSTRIAL R&D ON PRODUCTIVITY OF THE U.S. PRIVATE BUSINESS ECONOMY

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I. Introduction

This paper reports some of the results of ongoing research in a series of related subjects dealing with the timing and incidence of the effects of industrial R&D on productivity growth in the United States. Specifically, the subject covered here is the estimation at the macroeconomic level of the time lags involved in the relationship between R&D and productivity growth. Conceptually, these macroeconomic estimates include the total effects of R&D on the productivity of all industries in the private business economy, i.e., the direct effects and the interindustry spillover effects. However, they do not include the international spillover effects.

An analytical model for estimation of the lagged effects of industrial R&D expenditures on changes in productivity is specified, certain aspects of these relationships are discussed and illustrated, and empirical estimates of the distributed lag structure are presented and discussed. The resulting lag structure can be used to construct a time series of the R&D capital stock of the U.S. private business economy.

II. The Time Lag from R&D to Productivity

The results of R&D activities performed in a given year usually do not become available for effective use in production until a number of years later. Hence, the estimation of the completed capital stock of R&D which is available for production requires lagging of the past R&D expenditure over an appropriate embodiment period. This period includes the time lag between the average R&D expenditure and the development of the resulting producer good innovations, the time lag of the diffusion of these innovations in the industries using them, and the time lags between the adoption of these innovations and their effective utilization. This total lag period is estimated here empirically.

The Embodiment Lag Model for R&D*. The embodiment lag is estimated by extending the marginal product of the R&D capital model, which has been used to estimate the productivity effects of R&D for some time. (For recent descriptions see Griliches and Lichtenberg, 1984; and Terleckyj, 1982.) In this model, output is a Cobb-Douglas production function of labor and capital with an additional disembodied productivity growth trend at the rate, (λ) and a factor-neutral contribution of research capital, R, given by its own elasticity.

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* This model was originally developed its present form in the paper "A Growth Model of the U.S. Communication Industry" given at the annual meetings of American Economic Association, December 19, 1982. That paper is scheduled for publication in the volume Information and Communications Economics: New Perspectives, Meheroo Jussawalla and Helene Ebenfield (eds.) sheduled for publication by the North-Holland Publishing Co. in 1984, as Volume V of its Information Research and Resource Reports Series.

parameter, (α):

$$(1) \qquad Q = Ae^{\lambda t} L^{\beta} K^{(1-\beta)} R^{\alpha}$$

Productivity is defined as follows:

$$(2) \qquad P = Q/L^{\beta}K^{(1-\beta)} = Ae^{\lambda t}R^{\alpha}$$

Differentiating this function with respect to time gives the equation for productivity growth in which the growth rate of productivity is the sum of the disembodied autonomous growth term and growth in the stock of R&D capital times the elaticity of output with respect to the R&D capital stock:

$$(3) \qquad p = \frac{\dot{P}}{P} = \lambda + \alpha\frac{\dot{R}}{R}$$

If the depreciation rate for R&D capital is zero (or small enough to be ignored) the elasticity term in Equation 3 can be replaced by a term containing the marginal product of R&D capital as follows:

$$(4) \qquad \lambda + \alpha\frac{\dot{R}}{R} = \lambda + \frac{\partial Q}{\partial R} \cdot \frac{R}{Q} \cdot \frac{\dot{R}}{R} = \lambda + \frac{\partial Q}{\partial R} \cdot \frac{\dot{R}}{Q}$$

In this equation, the growth rate of productivity is specified as the sum of the disembodied term and the marginal product or R&D capital times the ratio of the increment in the R&D capital to output. The available results of empirical research indicate that the depreciation rate of R&D as a factor of production is either zero or small enough to be empirical indistinguishable from zero with the present data. (Griliches and Lichtenberg, 1984; Terleckyj, 1982.) The data favor zero depreciation of R&D over positive depreciation rates, at least rates large enough (on the order of 10 percent or more) to appreciably change the estimates of the net R&D investment. Thus, while R&D is subject to obsolescence, unlike labor and fixed capital, it does not wear out by use in production and the passage of time, and need not be replaced for those reasons. Hence, it should not be depreciated in productivity analyses (Terleckyj 1984).

The Equation 4 is timeless. It does not specify the period over which the R&D expenditures become a part of the R&D capital which is available for production. In the absence of an embodiment lag, and with a zero rate of depreciation of R&D, the increment in the R&D capital over a time interval would equal the R&D expenditure during this interval. However, the R&D capital resulting from a given R&D expenditure is not immediately available in production. In fact, the available research results suggest that the embodiment lags for R&D are quite long. Because of its length, the embodiment lag of R&D needs to be recognized explicitly in analyzing the effects of R&D on production and productivity. To this end the R&D capital stock in period t is specified as follows for discrete periods (assuming no depreciation of R&D):

$$(5) \qquad R_t = W_o E_t + W_1 E_{t-1} + \ldots + W_a E_{t-a} = \sum_{s=o}^{a} W_s E_{t-s}$$

Here the coefficients Ws, $0 \leq W_s \leq 1$, represent the proportion of the expenditure, E_{t-s}, of the period (t-s) which had already been embodied in the R&D stock, and (t-a) is the first period in which an R&D expenditure was made. The values of the embodiment coefficients Ws rise cumulatively from zero in the period sufficiently recent so that none of the R&D expenditure is yet embodied in the R&D capital stock to one in all periods sufficiently far in the past so that the R&D expenditures made in these periods are fully embodied in the R&D capital stock. Because the depreciation rate of R&D capital is assumed to be zero, all past expenditures eventually remain fully embodied in Rt. (A positive depreciation rate for R&D would require a relatively simple addition of depreciation terms to the present formulation.) The change in the capital stock defined in Equation 5 is then given as follows:

$$(6)\quad \Delta R_t = R_t - R_{t-1} = \sum_{s=o}^{a} W_s E_{t-s} - \sum_{s=o}^{a} W_s E_{t-s+1} = W_o E_t + \sum_{s=1}^{a} (W_s - W_{s-1}) E_{t-s}$$

Here the changes in the capital R&D stock over the time interval from t-1 to t is the weighted sum of current and past R&D expenditures with the weights, $v_s = W_s - W_{s-1}$ given by the differences in the embodiment coefficients for the successive past periods. For the current period t, the weight v_o equals the embodiment coefficient W_o. The positive values of the weights v_s are contained in a finite time interval representing an embodiment period of length a, and zero values otherwise. These weights can be estimated statistically by specifying a dynamic version of the Equation 4 assuming that the marginal product of R&D capital is constant over time. This version includes the expression for $\Delta R_t (\approx \dot{R}_t)$ from Equation 6:

$$(7) \qquad \frac{\Delta P_t}{P_t} = \lambda + \frac{\partial Q_t}{\partial R_t} \left[\left(W_o E_t + \sum_{s=1}^{a} v_s E_{t-s} \right) / Q_t \right]$$

The assumption of constant marginal product of R_t is justified on the ground that relative to the variance in v_s the trend values of the marginal product of R&D are likely to be stable and the cyclical and other short-term disturbances in it can be allowed for in the process of estimation by introduction of additional variables, or in other ways. Equation 7 cannot be estimated directly for a single time series because Q_t which is the denominator of the expenditure-output ratios would have to change each year with change in t, and a single series does not provide sufficient observations to estimate the weight structure. (It could be estimated when appropriate pooled cross-section time series observations are available.) Here, for a single sector model the equation is modified by multiplying the Equation 7 by the current year output, Q_t. This nets out the output denominator of the expenditure output ratio of the right hand side and changes the dependent variable from percentage change in productivity to the dollar value of the productivity change. This is shown in the Equation 8 below:

$$(8) \qquad Q_t \frac{\Delta P_t}{P_t} = \lambda Q_t + \frac{\partial Q_t}{\partial R_t} \sum_{s=o}^{a} v_s E_{t-s}$$

Equation 8 is used to estimate statistically the embodiment lag period for R&D and the aggregate stock of private R&D capital.

III. The Nature of the R&D-Productivity Lag

The process by which industrial research and development activities influence productivity growth consists of a number of distinct stages and probably involves a long period of time. The processes of conversion of industries to technologies embodied in new capital goods can be quite long. Mansfield (1968) found that the diffusion periods for many major innovations ranged between 10 and 20 years. As is illustrated below, even the conversion of the U.S. commercial airline fleet from 20 to 80 percent jet aircraft, which was accomplished in a relatively short time under conditions of rapid industry growth and with comparatively short depreciation lives of equipment, has taken nine years.

Before the actual diffusion period of innovations there are time lags between average R&D expenditure and the resulting innovation. There also may be considerable time lags between diffusion of innovations and their productivity effects. These "absorption" lags may reflect the switch-over time, operation of duplicate or parallel systems, training time and learning the use of new technology. Thus, at least in the case of some innovations, a total lag of 15 or even more years may exist between the expenditure of the average research and development dollar and completion of productivity changes generated by the resulting innovations.

It is not clear, however, whether the period over which the average R&D expenditure dollar influences productivity is of such length. A given shift in industry productivity often reflects results of R&D expenditures made by imitators as well as later R&D spending by the original innovators made in order to further improve their innovations. This would tend to reduce the time lags between R&D expenditure and productivity change. Imitation frequently has been relatively less time consuming than innovation. (Mansfield, Schwartz and Wagner, 1981.) Also, lesser innovations resulting from the short-term R&D are likely to have much shorter time lags. For these reasons the lag structure of productivity changes resulting from R&D expenditures may be shorter than the observed total diffusion cycle and other lags of major innovations.

The typical diffusion curves that have been established for innovations by Griliches for agriculture (1957) and by Mansfield for industry (1968) follow an S-shaped time path with an initially slow rate of diffusion followed by a period of rapid diffusion and eventually by diffusion at slower rates as a saturation ceiling is approached. One would expect the periods of accelerated productivity growth to correspond, perhaps with some lag, to the periods of rapid diffusion of innovations.

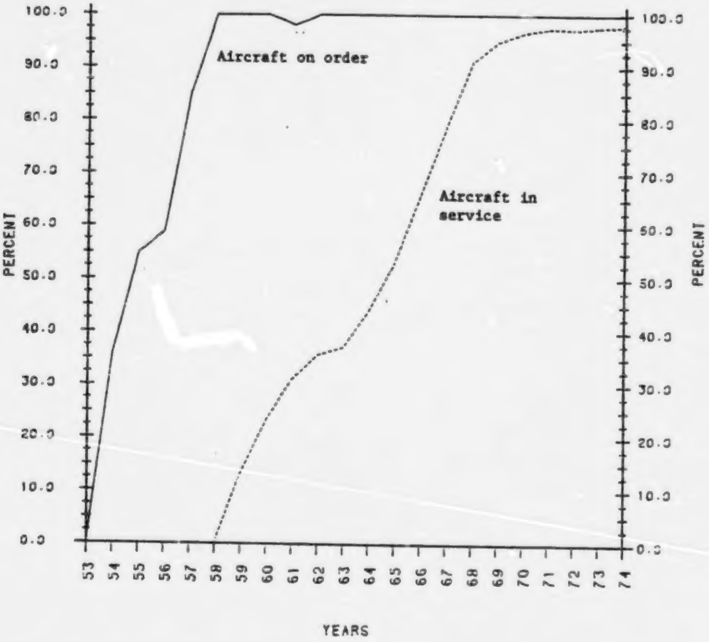
In case of process or intermediate goods innovations, (such as different rates of heating and cooling or a better grade ceramic), the period of market diffusion of an innovation, or a family of innovations, generally should correspond to the period of productivity improvements resulting from this innovation. If the new product is a capital good, e.g., a new airplane or computer, two diffusion processes are involved: first, in the market for this kind of capital and then in the capital stock or productive capacity of the industry using this type of capital. The two diffusion processes can have very different time profiles. Productivity effects depend on the capacity conversion process. They continue after the capital goods market is saturated until the entire capital stock of the consuming industry is changed over to the new equipment. After this time the contribution of this particular innovation to productivity growth ceases, but the higher level of productivity achieved with the new technology can be maintained with the same type of equipment.

Some of these processes can be illuminated by examining a major case of a capital innovation: introduction of jet aircraft in the U.S. commercial aviation. Chart 1, shows the diffusion of jets in the U.S. market for new aircraft for airlines and in the stock of aircraft fleet in use by the U.S. airlines. The two diffusion curves have different dates and time slopes. Within five years of introduction of jets in 1953, 100 percent of the aircraft on order by the airlines was for jets. On the other hand, it took nine years from the introduction of jets into airline service before the jets constituted 90 percent of the airline fleet.

Chart 2 shows the annual changes in the proportion of the airline fleet consisting of the jet aircraft together with the changes in the total factor productivity of the airline industry. Different types of jet aircraft that were introduced during the period are treated here as a single innovation. The productivity changes are derived from a Cobb-Douglas index of total factor productivity ("value of fleet" version) developed for the United States domestic airline industry by Lenz, Machnic and Elkins (1981). Since the output series used in constructing this index is based on seat miles flown, no adjustment needs to be made for cyclical capacity utilization, (the load factor). However, the annual productivity changes are subject to other short-term influences. In order to smooth out some of these short-term variations, the annual changes in the total factor productivity were converted

CHART 1

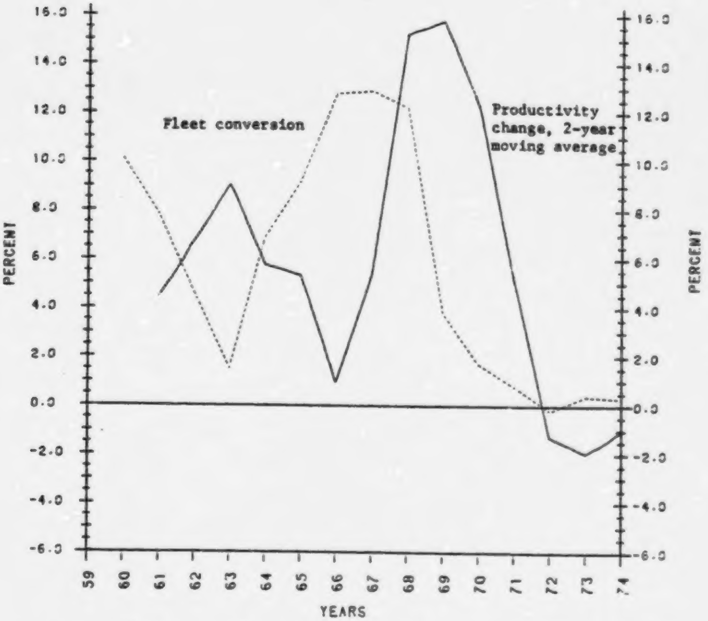
JETS AS PERCENT OF ALL AIRCRAFT
U.S. DOMESTIC AIRLINES



Source: Air Transport Association of America, Air Transport Facts and Figures, annually, 1953-1975.

CHART 2

ANNUAL CHANGES IN PERCENT OF FLEET IN THE JET AIRCRAFT AND IN TOTAL FACTOR PRODUCTIVITY, U.S. AIRLINE INDUSTRY, 1960-1974



$$\text{MAPCHANGE} = -2.81 + .74\text{CONVERSION}(-2) + .59\ \text{CONVERSION}(-3)$$
$$(t=2.01)\quad(3.22)\qquad\qquad(2.48)$$

$R_c^2 = .84$

D-W = 1.94

Sources: Fleet conversion: Air Transport Association of America, Air Transport Facts and Figures, Washington, D.C., annually, 1959-1975; Productivity: Lenz, Machnic, and Elkins, The Influence of Aeronautical R&D Expenditures Upon the Productivity of Air Transportation, University of Dayton, Research Institute, 1981, p. 171, Col. 12.

to a two-year moving average (MAPCTFP).

The distributed lag correlation between these two annual difference series is remarkably strong. Of the several lag estimates that were examined, the following percentage difference equation provides the best statistical expression of this relationship (t-ratios in parentheses):

$$(9) \qquad \text{MAPCTFP} = \underset{(2.01)}{-2.81} + \underset{(3.32)}{.74}\ \text{PCFLEET}(-2) + \underset{(2.48)}{.59}\ \text{PCFLEET}(-3)$$

$$R2(\text{corr.}) = .84; \quad DW = 1.94$$

Because the annual percentage changes in total factor productivity are defined here as a two-year moving average, the two lagged terms for the percent of fleet converted to jets during the year (PCFLEET) in this equation represent lags of 1.5 and 2.5 years, respectively. These results suggest that there has been a considerable time lag between the diffusion of this particular innovation and productivity changes of the industry in which it was adopted. However, the specific length of the time lag between introduction of new types of aircraft in service and increases in airline productivity would require more detailed specification of types of aircraft, because in addition to the absorption costs and lags in the airline industry, there have been important design improvements by the aircraft manufacturers which may have influenced the observed correlation (Mowery and Rosenberg, 1982).*

IV. Estimation of R&D — Productivity Lags

The Data. In order to estimate a lag structure distributed over a period of 10, 15 or possibly more years, long time series of data for both productivity and for R&D expenditure are required. The length of the series for the R&D expenditure must exceed the length of the productivity series being analyzed, enough to provide sufficient degrees of freedom for statistical estimation. A consistent series of total factor productivity data for the U.S. business economy was estimated by Kendrick and Grossman (1980) for the years beginning 1948 and reported subsequently by the American Productivity Center. The National Science Foundation data on private expenditures for industrial research and development are available only for the years beginning in 1953. However, an earlier work by the writer (Terleckyj, 1963) provided current dollar estimates of expenditures for R&D performance in industry for benchmark years 1921, 1927, 1931, 1933, 1938, 1940 and 1946 for which industrial directories were published and the research employment data was already compiled by earlier studies (Perazich and Field, 1940). These and other estimates were used to develop an annual series for the years 1921-1952 which was linked to the NSF series beginning in 1953. The estimates for years before 1953, were made as follows:

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* Also, more research is required to analyze the history of productivity changes in the U.S. airline industry during this period. For example, the implication that in the absence of conversion to jets the productivity of the airline industry would have been declining at nearly 3 percent a year during those years, would need to be examined by introducing additional variables into the equation, e.g., for the quality of service and for density of routes and flight schedules both of which apparently increased during this period, perhaps in the context of a more complete multiequation model of the airline industry.

First, an adjustment was made to exclude government-financed R&D performed in industry. It was assumed that in all years before 1940 one hundred percent of the expenditure for R&D performed in industry was made by the industry, and that the proportion of company-financed industrial R&D was 95 percent in 1940, and 80 percent in 1946. The actual proportion reported by the National Science Foundation for 1953 was 50 percent. Up to 1940, the data for missing years was interpolated geometrically, assuming constant growth rates between the benchmark years. For the war years 1941-45, the series was interpolated by assuming that the private industry R&D expenditure in these years deviated from an exponential 1940-46 trend by the same relative amounts as the patent applications, reported in a U.S. Department of Commerce data compendium (1973), deviated from their own trends for the same years. The data for the years from 1947 to 1951 were obtained from a U.S. Department of Defense series which was also reported in this compendium. The 1952 expenditure was interpolated geometrically between 1951 and 1953. All the numbers before 1947 were raised by 2 percent in order to allow for the expenditure by industry for R&D performed outside industry, that is, universities and nonprofit institutions.

The resulting series, in current dollars, is listed in Table 5-1.

Statistical Results. In estimating the distributed lag structure the R&D expenditure data was converted to 1972 dollars, using the GNP deflators to match the values of the dependent variable.

TABLE 5-1

TOTAL EXPENDITURE BY INDUSTRY FOR R&D PERFORMANCE, UNITED STATES, 1921-80

(in millions of current dollars)

Year	Expenditure	Year	Expenditure
1921	92	1951	1,300
1922	99	1952	1,708
1923	107	1953	2,245
1924	115	1954	2,373
1925	124	1955	2,520
1926	133	1956	3,343
1927	143	1957	3,467
1929	158	1958	3,707
1929	175	1959	4,064
1930	193	1960	4,516
1931	214	1961	4,757
1932	187	1962	5,123
1933	163	1963	5,456
1934	184	1964	5,887
1935	207	1965	6,548
1936	233	1966	7,328
1937	263	1967	8,142
1938	296	1968	9,005
1939	334	1969	10,010
1940	378	1970	10,444
1941	355	1971	10,822
1942	324	1972	11,710
1943	346	1973	13,293
1944	462	1974	14,878
1945	647	1975	15,820
1946	857	1976	17,694
1947	1,050	1977	19,696
1948	1,150	1978	22,491
1949	990	1979	26,028
1950	1,180	1980	30,400
		1981	34,385

Sources: 1921-1947, Terleckyj, Research and Development: Its Growth and Composition, National Industrial Conference Board, Studies in Business Economics, Number 82, New York, 1963, p. 104, modified as explained in the text.

1947-1951, Long Term Economic Growth 1860-1970, Series B-56, p. 221, U.S. Department of Commerce, Bureau of Economic Analysis. 1952 was interpolated.

1953-1981, National Science Foundation: 1953-1969, National Patterns of R&D Resources, NSF-77-310, Table B-5, p. 30; 1970-1980, National Patterns of Science and Technology Resources 1982, NSF-82-319, Table 5, p. 28.

It became necessary to stabilize the estimates of Equation 8, for cyclical and trend factors. A number of alternative formulations were explored. The results reported here were based on the following estimating Equation (10) which includes the current year and the past year values of output of the private business economy (Q(0) and Q(-1)) and of unemployment (UHH(0) and UHH(-1)) and also a series representing the estimated number of computers in use in the United States lagged three years, COMP(-3). The latter series, obtained from the data compiled by Phister (1979) was included in order to compensate in part for the understatement of real capital input on which the total factor productivity data is based. The existing U.S. statistics of production, investment, and capital stock are based on the computer data not deflated for the large declines in computer prices which have occurred over the past thirty years. The three year lag used for computers in Equation (10) corresponds to the absorption lag estimated in the preceding section for the jet aircraft.

$$(10)\quad Q_t \frac{\Delta P_t}{P_{t-1}} = b_0 + b_1 Q(0) + b_2\, Q(-1) + b_3\, UHH(0) + b_4\, UHH(-1)$$

$$+\, b_5\, COMP(-3) + \sum_{s=}^{a} L_s E_{t-s}$$

Here the distributed lag coefficients $L_s = mv_s$ with $m = \frac{\partial Q}{\partial R}$ the marginal product of R&D capital assumed to be constant and the v_s representing the embodiment lag coefficients, $\sum_{s=0}^{a} v_s = 1.0$. Q_t and E_t are expressed in billions of 1972 dollars and the unemployment, UHH is in thousands. The number of computers is also in thousands.

The present formulation of Equation 10 is preliminary. At later stages of this project attempts will be made to stabilize the estimates for cyclical variation by independent variables other than output and also to adjust both the capital stock and the output of the private business economy for real quantity of computers using some experimental heuristic deflators which are being developed. Failure to adjust the computer output for the large decline in computer prices probably represents the largest single distortion in the U.S. economic output data at present. It is clearly very important to consider the possible implications of this distortion for the results of statistical analyses of productivity changes.

The current distributed lag analysis, does not provide a clear choice of a particular estimating approach. If sufficient independent information is available, the properties of a lag structure can be specified and imposed a priori. The parameters of the lag function can then be estimated statistically from the data. The two basic properties of the lag function are the length of the time period over which the productivity effects of R&D are distributed and the shape of the relative magnitude of these effects. From the preceding discussion it appears that the time lags between the expenditure of the average R&D dollar and its productivity effect may be quite long. Regarding the shape of the lag function, no prior restriction appears safe. For a single R&D project and even for families of related innovations a single peak lag distribution may be justified. However, it is not clear whether the actual aggregate series consisting of numerous R&D activities with different maturation periods may have a different structure, or hardly any structure at all. All one can reasonably infer from prior knowledge is that in the aggregate, R&D effects on productivity have been positive. Other attributes of the lag structure: existence of a delay period before R&D effects begin, skewness of the distribution with a relatively long tapering off period, a single peak, etc. may appear more or less plausible but cannot be imposed in advance. Therefore, the search for the distributed lag structure is conducted here with methods which do not impose severe restrictions on the results.

The length of the lag period and the shape of the lag structure were explored by estimating a series of lag structures for periods varying in length from 10 to 20 years, and by varying the data periods. A fifth degree polynomial lag and the less restricted lag methods developed by R. J. Shiller (1973) were used in addition to the unrestricted OLS estimates.

The statistical results are best described by the 15-year lag structures shown in Table 5-2 which includes OLS, fifth degree polynomial and first order of smoothness Shiller lag estimates based on productivity change data for the period 1955-81.

The lag structures stabilized for the cyclical variables estimated for periods of varying length all had positive sum of lag coefficients and a relatively long lag period. Lag structures restricted to ten years ended with relatively large coefficients. On the other hand, lag structures fitted over periods of 15 to 20 years had similar results. Generally, the results obtained by the three distributed lag methods were similar, although the sum of the coefficients has varied more than the shape of the lag structure.

TABLE 5-2

ESTIMATES OF A LAG STRUCTURE OF R&D EFFECTS ON THE REAL VALUE OF ANNUAL INCREMENTS IN PRODUCTIVITY OF THE U.S. PRIVATE ECONOMY
(Based on data for 1955-81. Value of output in 1972 dollars)

Variable	Ordinary Least Squares		Polynomial of 5th Degree		Shiller Lag	
	Coefficient	t-ratio	Coefficient	t-ratio	Coefficient	t-ratio
Constant	481.50	6.29**	231.71	2.74*	304.53	3.90**
Q(0)	.53	6.98**	.62	6.94**	.58	6.90**
Q(-1)	-1.63	15.67**	-1.16	9.78**	-1.29	11.92**
UHH(0)	.0016	.75	.0049	1.60	.0036	1.39
UHH(-1)	- .0195	9.96**	- .0121	4.17**	- .0135	5.92**
Computers(-3)	.059	2.12	.059	1.05	.048	1.13
R&D Expenditures Years Lagged						
0	7.35	3.46*	1.46	1.25	3.59	1.56
1	1.90	1.01	- .67	1.21	.23	.15
2	-10.93	4.77**	- .64	.82	-3.16	1.63
3	- 5.31	3.52*	.37	.68	-1.75	1.19
4	6.63	4.48**	1.60	4.21**	2.33	1.61
5	5.79	2.39	2.60	5.62**	4.73	2.23*
6	13.95	6.32**	3.20	6.58**	5.76	3.13**
7	1.34	.93	3.41	8.35**	3.41	2.56*
8	2.95	1.57	3.36	8.99**	2.25	1.67
9	5.71	3.18*	3.25	7.17**	3.26	2.12*
10	9.72	4.27**	3.23	6.37**	5.69	3.11**
11	11.06	5.20**	3.38	6.59**	6.60	3.30**
12	7.53	4.01*	3.60	5.98**	5.15	2.48**
13	4.49	1.73	3.56	4.99**	3.14	1.51
14	10.97	5.79**	2.62	4.60**	3.67	2.20*
15	2.09	1.41	- .22	.15	1.92	1.09
R^2 Corrected	.994		.970		.977	
D-W Statistic	2.74		2.59		2.27	
Sum of Lag Coefficients	75.24		34.11		46.82	

* Statistically significant at 5 percent level.

** Statistically significant at 1 percent level.

As is typical of the OLS lag estimates, the individual coefficients vary widely but the sum of the coefficients significant at the 95 percent level is positive and the sum of all coefficients is positive. The fifth order polynomial lag structure is much less erratic than the OLS structure. It has significant positive coefficients at the years -4 to -14 with all other coefficients insignificant and with the sum of insignificant coefficients near zero. The Shiller lag estimates are quite similar to the estimates obtained by fitting the fifth degree polynomial. The five year moving average of the OLS coefficients has a pattern similar to the polynomial lag structure but implies a longer mean lag.

V. Some Inferences and Questions for Further Research

The lag pattern suggested by the present estimates is quite plausible. This pattern implies an average delay period of 3 years after R&D expenditure before the resulting innovations begin to raise productivity. These results are qualitatively and quantitatively similar to the results obtained earlier for the communication industry (Terleckyj, 1984B). However, implications of use of different lag structures and their sensitivity to differences in the productivity data is being examined now and the results will be reported later.

The sum of the estimated lag coefficients theoretically equals the marginal product of R&D capital within the assumptions of the model used. However, in view of the uncertainties involved in the data and in the estimating procedure utilizing cyclical and trend variables, no inferences about the absolute magnitude of these effects or the productivity rates of return they imply seems warranted at this time.

The results obtained here may provide a better indication of the general length and shape of the lag structure than of the absolute magnitude of the R&D effects. The time period and the shape of the lag structure have been more robust under different estimation techniques, with different treatment of other variables, with varying of lag period and with varying of the data period than has been the sum of the lag coefficients.

Elsewhere, I have argued (Terleckyj, 1984A) that different concepts of R&D capital requiring different estimation assumptions need to be constructed depending on the purpose of analysis. Accordingly, a stock of R&D capital, as an input in production in U.S. private business economy, using the embodiment lags from the polynomial lag structure has been constructed. It was found to have a significant correlation with the U.S. productivity consistent with the results reported earlier (Levy and Terleckyj, 1983).

In line with a Schumpeterian analysis one would expect the productivity raising effects of R&D to extend over much longer periods than the R&D effects on profits which would be worn out by competition sooner. The profits of the original producers of new producer goods, especially new capital goods, may be expected to decline in the later stages of the innovation cycle, as the imitators enter the competition in the market and as the prices of the new goods continue to weaken through increased competition, expansion of supply and eventually saturation of demand. Hence, both the embodiment lag structure and the depreciation of R&D as private capital of R&D investors and as the productive capital of the economy can be expected to be quite different.

The relative (percentage) distribution of lagged effects of R&D on productivity obtained with the fifth degree polynomial structure (omitting the insignificant coefficients) is compared in Table 5-3 with the relative lag structure of the effects of R&D expenditures on the profits of companies making these investments estimated (though with different method and data), by Ravenscroft and Scherer (1982). The estimated profitability period of R&D expenditures appears much shorter than the period of their contribution to economic growth. Similar results regarding rapid obsolescence of private R&D capital were obtained by Pakes and Shankerman (1984). While the patterns of the two effects of R&D in Table 5-3 differ greatly, both support the existence of an initial delay period after the R&D expenditures. Qualitatively similar indications of this delay were also reported in the detailed case studies of private and social R&D rates of return to R&D. (Mansfield, et. al., 1977; Foster Associates, Inc., 1978; Robert R. Nathan Associates, Inc., 1978.)

At later stages of this research the results reported here will be tested further, and indicators of R&D capital stocks will be developed at the product field level. The importance of using empirical time lags in analyzing this relationship will be assessed. Another subject to be considered at the industry level is the comparison of results obtained with two alternative methods of tracing interindustry impacts of changes in R&D capital on productivity: the approach of the technology transfer matrix developed by Scherer (1984) based on allocation of individual patents from originating industries to utilizing industries and the approach involving the interindustry allocation of R&D in proportion to sales as given by the transaction flow and the capital flow input/output matrices. The possibility

TABLE 5-3

ESTIMATED LAG DISTRIBUTION OF THE EFFECTS OF R&D ON PRODUCTIVITY OF THE ECONOMY AND ON PROFITS OF COMPANIES MAKING THE R&D EXPENDITURES
(percent of total effect)

Year	Effect on Profits	Effect on Productivity
0	-	0
-1	1	0
-2	8	0
-3	21	0
-4	29	4.8
-5	25	7.7
-6	13	9.4
-7	4	10.1
-8	0	9.9
-9	-	9.6
-10	-	9.6
-11	-	10.0
-12	-	10.6
-13	-	10.5
-14	-	7.8
-15	-	0
Total	101	100

of existence of direct effects of government R&D on productivity of industries utilizing the products of industries performing government funded R&D will be examined with the data reflecting the observed time lags between R&D and productivity.

Important questions concern the quality of the input and output data underlying the estimates of total factor productivity. As mentioned earlier, probably the single largest distortion in this data is its failure to reflect the large declines in prices and hence the large increases in real quantity of computers that have occurred since the introduction of computers in the private economy. In this paper this issue was approached by introducing a time series of the estimated number of computers in use as an explanatory variable in the productivity equation. In the next stage, the real value of computer output and of the stock of computers in use will be introduced.

CHAPTER VI

SUMMARY

I. An Overview

This report contains the results of a series of analyses which explored the effects of government R&D expenditures on productivity growth of the private economy and on private R&D expenditures. These analyses take into account the differences in the objectives of the various government programs which involve R&D expenditures for research and development and attempt to identify the differences in their economic effects. These analyses consider the intercompany and interindustry impacts of government R&D as well as the interactions between the government R&D programs and policies and the private R&D choices. This report also presents an analytical model for estimating the time pattern of the effects of private R&D investments on productivity growth in the economy and a series of empirical estimates based on this model.

After a brief survey of the historical trends in government R&D activities, Chapter 1 reviews the results of previous research with regard to the effects of government R&D on productivity growth in the private sector and on private R&D expenditures. Generally, government R&D programs undertaken in support of productivity growth or technological development of an industry were found to have resulted in productivity improvements of that industry. The classic case is the U.S. agriculture. On the other hand, no direct effect was found of the government contract R&D on productivity of firms or industries performing this R&D. However, there are some indications that productivity growth of industries buying producer goods from the industries performing government contract R&D may have been somewhat enhanced.

With regard to the effect of government R&D on private R&D expenditures, the evidence seems to be much clearer. Specifically, there are strong indications that a direct positive relationship exists between the real volume of government contract R&D performed in industry and the real private industry expenditure on research and development. A considerable number of different studies examined this relationship at the level of products, firms, industries and economy and found such positive correlation. While negative effects have been observed, in most cases the relationship was found to be positive. The research performed in the study reported here in Chapter 2 found that the total effect of government contract R&D on private R&D spending estimated at the level of the economy was considerably larger than the effects estimated by other researchers at the level of industries or companies. This suggests that there may exist considerable interindustry, intercompany, and interproduct cross effects stimulating private R&D outlays. Thus, following the original adaptation of the results of government contract projects to private market products further R&D expenditures by the original innovators may be induced as well as R&D spending by imitators and developers of subsequent applications of these products in other private markets.

II. Government Contract R&D and Private R&D Spending in Industry

Examination of the relationship between government contract R&D and private R&D expenditures at the macroeconomic level is reported in Chapter 2. (These results were originally published in Levy and Terleckyj, 1983.) Particular attention is given to the possible role of the reimbursement of government contractors for some of their private R&D expenditures through an overhead allowance on the contract amounts. In addition, this chapter contains the data and some further discussions of the overhead reimbursement of government contractors for R&D.

The economic analysis is performed in the context of the macroeconomic U.S. investment function for private industry R&D expenditures. Government expenditures for R&D are introduced into this function as an independent variable determining the private industry investment in R&D along with the tax variables, output, unemployment, and the age of the stock of the R&D capital. Consistently large and highly significant effects of government R&D spending on private R&D expenditures are obtained. However, some important distinctions need to be made with regard to the kind of government R&D expenditure.

When the total government R&D expenditures is included in the investment equation for private R&D, the coefficient for government R&D is highly significant and has the value of .21. This can be interpreted to mean that an additional 21 cents of private R&D outlays is associated with a dollar of government R&D spending. However, when the total government R&D expenditure is separated into government contract R&D and all other government R&D (i.e., the R&D performed in government, in universities and in nonprofit institutions) a larger coefficient is obtained for government contract R&D, at .28, while the coefficient for all other government R&D is very small and not statistically significant.

A search was conducted for time lags in this relationship. No lags could be identified for the government contract R&D performed in industry. However, when the data for other government R&D was lagged three years, a coefficient significant at 10 percent level was found for it which had the value of .19. The coefficient for government contract R&D remained virtually unchanged at .27 and remained highly significant. Further disaggregation in other government R&D was not pursued in this analysis.

It is important to ascertain whether the strong correlation observed between government contract R&D and private R&D may be due simply to the way the R&D statistics are defined. The government contractors, specifically the Department of Defense and NASA contractors, are allowed to charge as part of the overhead cost on all government contracts, including procurement as well as R&D contracts, certain company expenditures for R&D. This R&D expenditure appears in the NSF survey data as private R&D outlays while in effect it is paid for, indirectly, by the government. This practice of contractor reimbursement of R&D, also known as IR&D (for independent R&D), as a normal cost of doing business originated in the early 1960s after government contracting practice changed from "cost plus" to fixed price contracts.

Because of the possibility that the correlation between government contract R&D and private R&D spending may be influenced spuriously by the IR&D payments to performers of the R&D contracts, IR&D was removed from the statistical relationship in the following manner. The data for IR&D originally compiled by Reppy (1976) and Shapley (1980) was extended for later years with the data obtained from government agencies, converted to calendar years and expressed in constant dollars. These amounts of IR&D were then subtracted from the private R&D spending and were introduced into the equation for private R&D as a separate independent variable.

In the modified investment equation the IR&D variable had a negative coefficient which was not significant, but it had a value near unity. The estimate of the effect of a contract R&D on private R&D was unchanged at .27. While these results do not fully explain the economic role of IR&D they suggest that it may be a substitute for direct private expenditures by the contracting companies, but the basis for this inference is not very firm. What appears to be quite clear, however, is that the positive relationship observed between contract R&D and private R&D spending is not in any way due to the practice of reimbursement of defense and space contractors for some of their R&D spending as part of an overhead allowance on government contracts.

III. Direct Effects of Government R&D on Productivity Growth

Analysis of the overall effects of government R&D as well as of private R&D on productivity were examined at the macroeconomic level by means of a productivity function estimated for the years 1948-81 for the private business economy. No direct estimate of lags of government R&D on productivity could be obtained, but the same six year lag was used for government R&D as was used for the private R&D to generate the R&D capital stocks. Consistently significant and stable estimates of the effects of fixed capital per hour (K/L), of unemployment (UHH), and of the ratio of the stock of private R&D capital to fixed capital (RDSTOCK/K) were obtained. For total government R&D no significant estimate could be identified. However, when the government contract R&D was separated from all other government R&D, a significant effect was identified for government contract R&D stock relative to the fixed capital stock (GCRDSTOCK/K), but not for all other government R&D. The following productivity function was estimated by the generalized least squares method:

$$\begin{array}{lllll} \ln(Q/L) = & 2.14 + & 2.56\ \ln(K/L) & + .258\ \ln(\mathrm{RDSTOCK}/K) & \\ & (6.14) & (1.99) & (3.82) & \\ & + .065\ \ln(\mathrm{GCRDSTOCK}/K) & - .0095\mathrm{UHH} & & \\ & (2.26) & (2.14) & & \end{array}$$

($\bar{R}^2_c$ = .99, D-W statistic = 1.91; the t-ratio for the residual autocorrelation coefficient = 1.37.)

These results are interesting because for the first time they provide statistical evidence of possible existence of a direct effect of government contract R&D on private productivity, at least at the level of the economy as a whole. Therefore, they should be tested in subsequent research. Perhaps an interindustry analysis could help identify the mechanism by which the government contract R&D may effect the productivity growth of industries directly.

The above productivity function was used to analyze the productivity slowdown between the periods 1948-73 and 1973-81 in the context of a growth accounting analysis performed by E. Denison (1984). Denison's framework does not explicitly identify the effects of R&D on productivity which are included in the unexplained residual. Calculations based on the above equation indicate that changes in the growth of the stocks of private R&D capital and of government contract R&D capital, could account for about one-third of this unexplained residual. Specifically, the declines in the relative growth of the private R&D stock can explain a change of -.25 in the annual productivity growth between these two periods. The decline in the relative growth of the government contract R&D stock can explain a change of -.30 percent. The estimated elasticity of productivity with respect to the R&D capital ratio in the above productivity equation is four times greater for the private R&D

stock than for the government R&D stock. However, because of the very sharp decline in the growth rate of government R&D stock, its total calculated effect on productivity growth is greater than the effect of the relatively milder slowdown in growth of private R&D stock. (Because these calculations were based on simulation of the entire NPA economic growth model—see Terleckyj, Holdrich and Levy, 1982—these results cannot be derived directly from the formula.)

IV. Specific Policy Issues Regarding Support of Private R&D

Chapter 4 considers the effects of different government policies undertaken in support of private industry R&D investments, examines the theoretical basis for government support of private R&D, and reviews some empirical evidence bearing on policy choices.

High Risk and High Cost of R&D. One line of policy considers R&D as a generally desirable form of investment which needs special public support. This need is based on the assumption that there is an insufficiency of investment in R&D relative to other types of investment because the risk and cost of projects exceeds the resources of individual firms. Government support of R&D across-the-board is needed to bring about a socially optimal allocation of investment resources. This support can be provided by general tax and subsidy policies. Such measures are equivalent to simple reductions in the cost of all R&D projects to private firms. Basic, and other supportive research conducted by the government can also be used to support private R&D. This government sponsored R&D can expand the range of potential private R&D projects and can also improve the choices of individual projects made by the private firms. All these government policies have the effect of making the private R&D expenditures more productive in terms of private returns.

Large External Returns to R&D. Another line of policies involves support of specific R&D projects or classes of projects on the grounds that the external benefits of these projects are large and the prospective private returns are insufficient for the market process to generate the socially optimal amounts of investment in these projects. Whether the market process leads to an efficient choice of R&D projects in terms of social returns depends on whether private returns are positively correlated with the external returns. If there is a strong positive correlation, then private project choices will also bring about the realization of external social benefits. Otherwise, potential social benefits will remain unrealized.

Data on private and social rates of return available for 53 cases of innovation from previous studies (Mansfield, et.al., 1977; Foster Associates, 1978; Robert R. Nathan Associates, 1978) were examined here for correlation between private and external (i.e., social less private), rates of return. No correlation was found between the two. To the extent that these cases may be typical, these results suggest that efficient private selection of innovation projects does not lead to an efficient selection of socially productive projects. This further implies that a different selection mechanism presumably one involving government selection of projects is required to realize the external benefits.

The proper choice of specific innovation or R&D projects for government support requires that they have high external rates of return but at the same time low private rates of return. Characteristics of innovation projects which may produce large social benefits and at the same time are not attractive for private investors include large total benefits (mass consumption), ease of imitation, small market shares of producers, large project costs, small unit profits and limited market information. A two-criterion model for government choice of projects is sketched out. The choice problem for government decision makers is much more complex and much more difficult than the choice of R&D projects by a private firm. They have to select "social winners" which are also "private losers" while the decision makers in the business sector need only to select "private winners." Hence, any application of a formal choicea model in government would require careful prior analysis and program development.

A Policy Gap? The present state of R&D policies in the United States is surveyed briefly. The general tax and subsidy support of R&D has been increased by the enactment in 1981 of the R&D marginal tax credit. Real private R&D spending has been rising rapidly since the mid-1970s. Also, the U.S. government has a number of programs for support of basic research. The level of real outlays in these programs is at least being maintained. Hence, it is unlikely that a large general policy gap in support of private R&D exists now in the United States. However, in view of the long-term escalation of consumer costs of energy and of health care, it may be that significant opportunities for productive government expenditures in support of innovation may exist in these two areas. Whether an R&D policy is more appropriate than other policies aimed at controlling social costs in these fields and, if so, how government R&D programs may be designed effectively for them remains to be determined.

V. Timing of the Effects of R&D Activities on Productivity of the Economy

An Analytic Model of the R&D Embodiment Lag. The timing of the effects of R&D on productivity growth is analyzed in Chapter 5. An analytical model for estimating a distributed lag structure of the effects of R&D on productivity of the economy is introduced and estimates of the embodiment lags of R&D expenditures in the R&D stock based on this model are presented. The estimated lag structure can then be used to generate stocks of R&D capital available in production. Macroeconomic estimation is adopted as the first basic step because at the level of total economy the intercompany, interproduct and interindustry effects of R&D on productivity resulting from diffusion of innovations produced by R&D in different utilizing industries would be captured which would not be captured by estimates made at the more detailed industry or company level.

The R&D embodiment lag model is developed as a dynamic extension of the standard R&D marginal productivity model which has been used for some time in estimating the effects of R&D on productivity in a single period. The one period model requires the assumption of constant marginal productivity of R&D expenditures in all uses. While clearly an abstraction, this generalization has proven fruitful in that it enabled researchers to obtain consistent and meaningful results. In the present extension of this analysis to multiple periods the assumption of constant marginal productivity of R&D over time is required. The multiperiod model permits estimation of the embodiment coefficients of R&D from the observed changes in total factor productivity as a function of past additions to R&D capital. In the present formulation the depreciation rate of R&D as productive capital is assumed to be zero, because the empirical evidence suggests that depreciation rate of R&D has been small or zero. However, the model could be readily modified to allow positive depreciation of R&D when needed.

The productivity changes are defined as a function of past real expenditures for research and development as follows:

$$Q_t \frac{\Delta P_t}{P_t} = \lambda Q_t + \frac{\partial Q_t}{\partial R_t} \sum_{s=o}^{a} v_s E_{t-s}$$

Here Q is the value of output, P is total factor productivity, R is the stock of R&D capital and E is the R&D expenditure during a period. The time period is indicated by the subscript t, the given embodiment lag period by s and the total length of the lag period by a. The v's, are the embodiment proportions applicable to the embodiment lag periods defined by s, and - is the autonomous (exogenous) growth rate in total factor productivitywhich does not depend on R&D.

The left hand side of the above equation represents the real value of the productivity increase in period t, while the right hand side consists of a term which represents the value of the exogenous productivity change and a term which is the product of the sum of the increments of past R&D expenditures embodied in period t multiplied by the (constant) marginal product of R&D capital.

Estimation of the R&D Embodiment Lag Structure. The embodiment lag structure was estimated with the NSF data for private industry R&D expenditures beginning in 1953 as extended back to 1921 by the author, and the total factor productivity data developed by John Kendrick and Elliott Grossman at the American Productivity Center for the years 1948-81. Because the annual productivity changes are subject to considerable cyclical influences it is necessary in order to obtain stable estimates of the embodiment structure to stabilize the estimates for cyclical change. This was done by the introduction of the lagged output and of unemployment variables into the estimating equation. Also, a series representing the number of computers in use in the United States was introduced. This was done because the available data for fixed capital stock seriously understates the real value of computers used. This is due to the failure of existent statistics to reflect the declines in computer prices that have occurred over the years.

The paper considers a number of characteristics of the innovation processes which determine the lag structure of the R&D effects. As a case in point, the relationship between the diffusion of jets in civil aviation in the U.S. and productivity changes of the U.S. airlines which reflects these innovations is examined. Very strong and consistent relationship, with a two-year lag is found between fleet conversion and productivity changes of the airline industry. The entire switchover period for this major innovation was spread over a period longer than ten years. Even longer time lags were found by other researchers earlier for innovations in industry and in agriculture.

However, the actual period of embodiment and all the other attributes of the lag function are approached empirically. Neither the length nor the shape of the expected lag structure can be imposed a priori for total R&D in the economy. Existence of such attributes as a single peak or a delay period between the R&D expenditures and the beginning of productivity improvements while perhaps plausible or even typical of major innovations does not necessarily characterize the lag function for total R&D. The only prior assumption that appears justified by the existing research results is that the sum of lag effects of R&D on productivity is positive. Because of the uncertainty about the other characteristics of the lag structure, methods of statistical estimation were adopted which impose relatively moderate *a priori* constraints on the results. These methods include the ordinary least squares (OLS) lag structure, a fifth degree polynomial lag structure, and the estimates made by the Shiller lag method.

Estimates were made for lag periods of ten, fifteen and twenty years and with the data periods beginning in 1949, 1956 and 1960 respectively, and ending in 1981. The best fit was found for fifteen year lag period estimated with the data for 1956-81. Ten year lag period appeared too short and the results for the 20 year lag were similar to those obtained for the 15 year lag period. The intermediate lag period was preferred because the data for earlier years (before 1953) involved interpolation and the annual distributed lag structure estimated for the larger data period would be influenced by these values. On the other hand, the shorter data period provides very few statistical degrees of freedom especially for the OLS estimates. Actually all three methods of estimation produced similar results.

The best fit lag structure had four years of delay before beginning of productivity effects and a relatively flat shape of positive effects extending from years -5 to -14. It is based on the 5th degree polynomial distributed lag estimate. However, the moving average of the OLS estimates and the Shiller lag structure were essentially similar. This best fit lag structure was used to calculate the capital stock of private R&D. When introduced into the productivity equation for the U.S. private business economy the coefficient of the R&D stock indicated highly significant contribution of R&D to productivity.

While the results obtained presently seem plausible, their further testing and verification are needed. In particular, it is desirable to test the sensitivity of these results with respect to the choice of the index of the total factor productivity measure and with respect to the methods used to adjust the estimates for cyclical variability.

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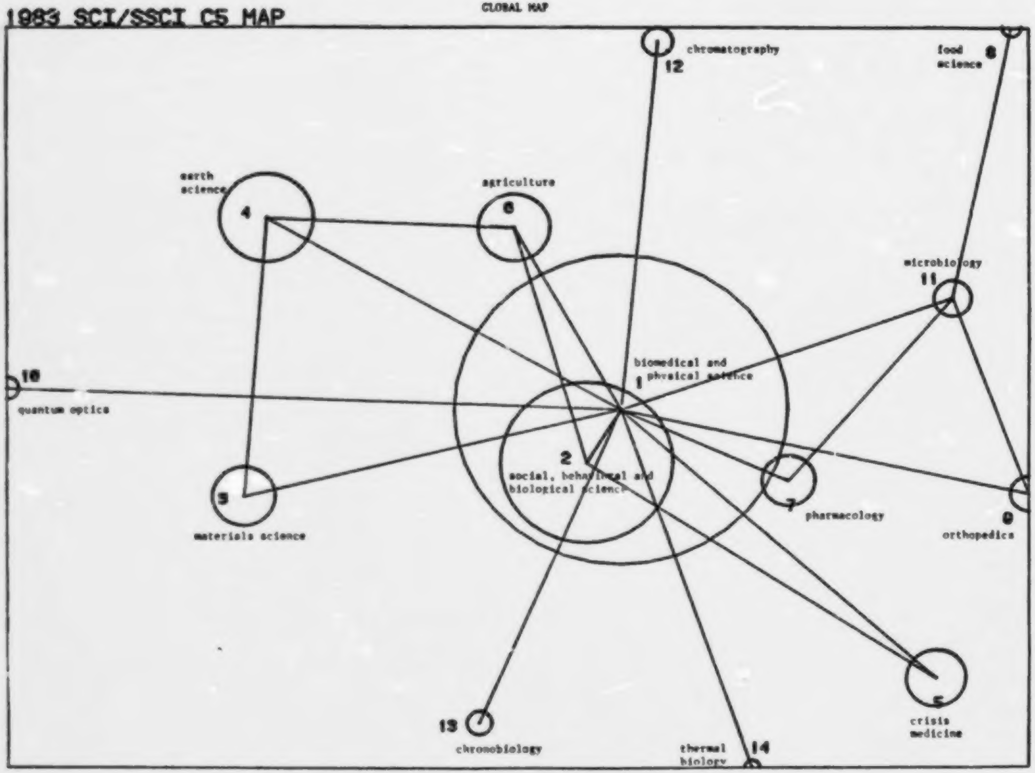
1983 SCI/SSCI C5 MAP

GLOBAL MAP



12
chromatography
food science
8
earth science
4
agriculture
6
microbiology
11
biomedical and physical science
1
10
quantum optics
2
social, behavioral and biological science
3
materials science
7
pharmacology
9
orthopedics
5
crisis medicine
13
chronobiology
thermal biology
14

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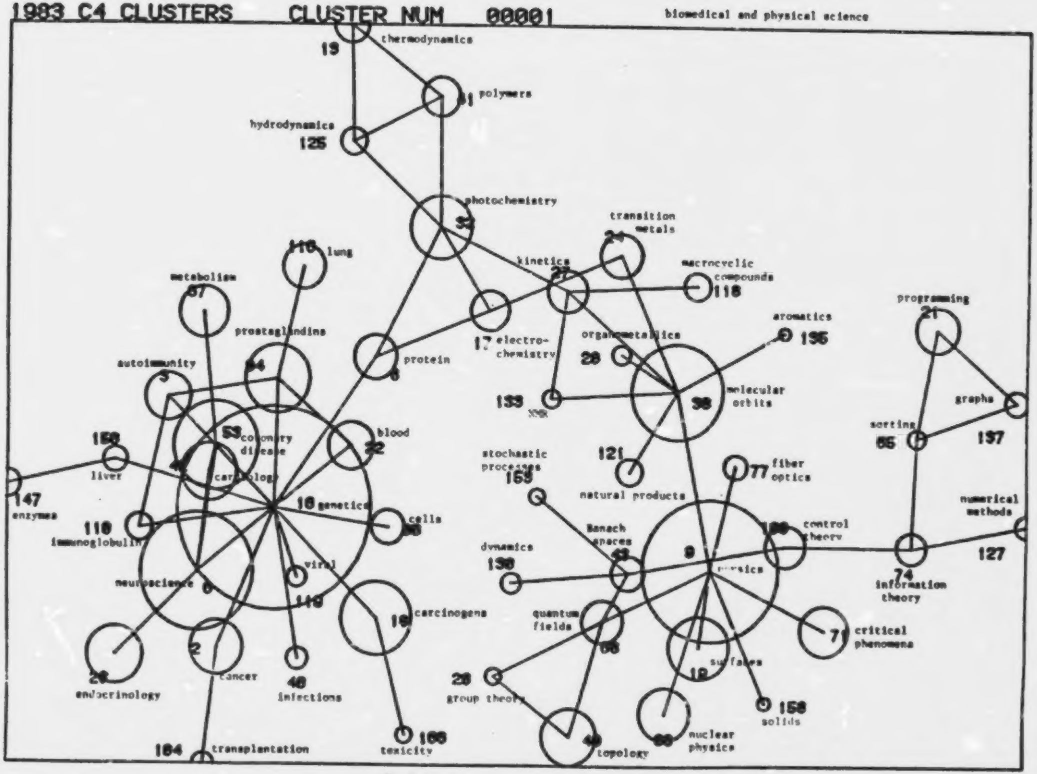
345

1983 C4 CLUSTERS CLUSTER NUM 00001 biomedical and physical science


19 thermodynamics
31 polymers
hydrodynamics 125
32 photochemistry
transition metals 24
110 lung
kinetics 27
macrocyclic compounds 118
metabolism 67
17 electro-chemistry
aromatics 135
programming 21
prostaglandins
organometallics
28
protein 8
autoimmunity 3
54
133 NMR
38 molecular orbits
graphs 137
sorting 65
150
53 coronary disease
blood 32
stochastic processes 153
121 natural products
77 fiber optics
liver
cardiology
147 enzymes
16 genetics
cells 96
numerical methods 127
118 immunoglobulin
Banach spaces 43
9 physics
100 control theory
dynamics 130
74 information theory
neuroscience 6
viral 119
quantum fields 66
18 carcinogens
critical phenomena 71
2 cancer
surfaces 12
26 endocrinology
48 infections
28 group theory
158 solids
104 transplantation
108 toxicity
49 topology
60 nuclear physics

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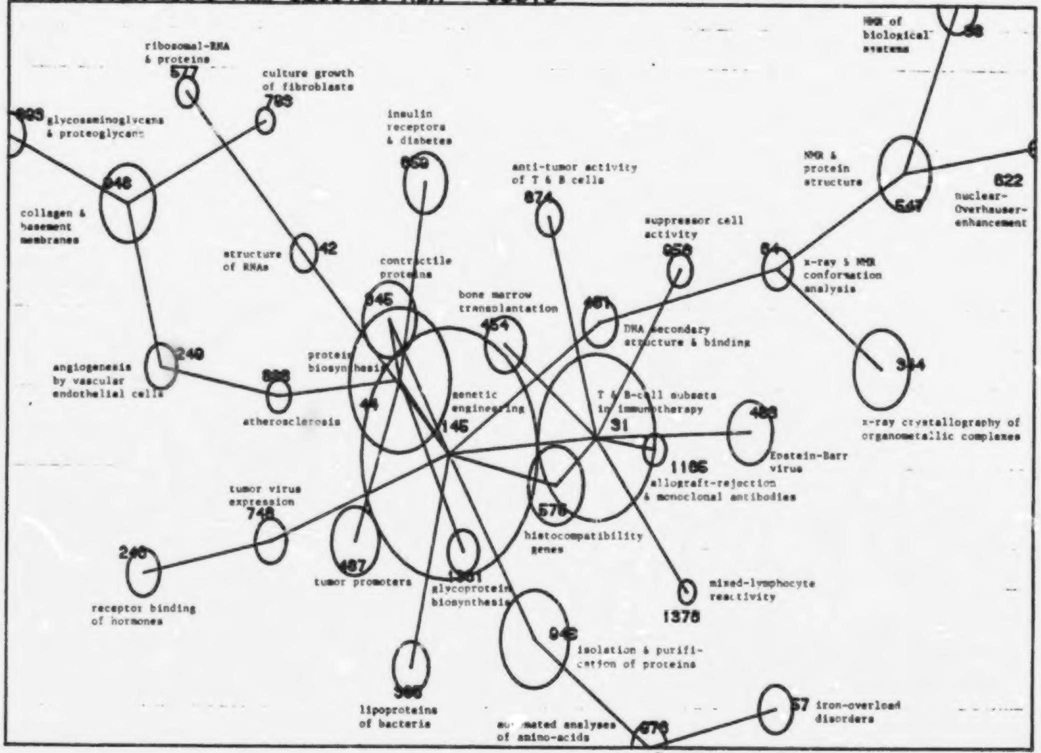
C3 CLUSTER MAPS 1983 CLUSTER NUM 00016 molecular genetics & immunology


NMR of biological systems
98
ribosomal-RNA & proteins
577
culture growth of fibroblasts
798
893
glycosaminoglycans & proteoglycans
insulin receptors & diabetes
659
anti-tumor activity of T & B cells
874
NMR & protein structure
547
822
nuclear-Overhauser-enhancement
946
collagen & basement membranes
suppressor cell activity
958
84
x-ray & NMR conformation analysis
structure of RNAs
42
contractile proteins
345
bone marrow transplantation
454
481
DNA secondary structure & binding
240
angiogenesis by vascular endothelial cells
888
atherosclerosis
protein biosynthesis
44
genetic engineering
145
T & B-cell subsets in immunotherapy
31
488
Epstein-Barr virus
344
x-ray crystallography of organometallic complexes
1186
allograft-rejection & monoclonal antibodies
576
histocompatibility genes
tumor virus expression
748
246
receptor binding of hormones
487
tumor promoters
1381
glycoprotein biosynthesis
mixed-lymphocyte reactivity
1378
948
isolation & purification of proteins
386
lipoproteins of bacteria
automated analyses of amino-acids
976
57
iron-overload disorders

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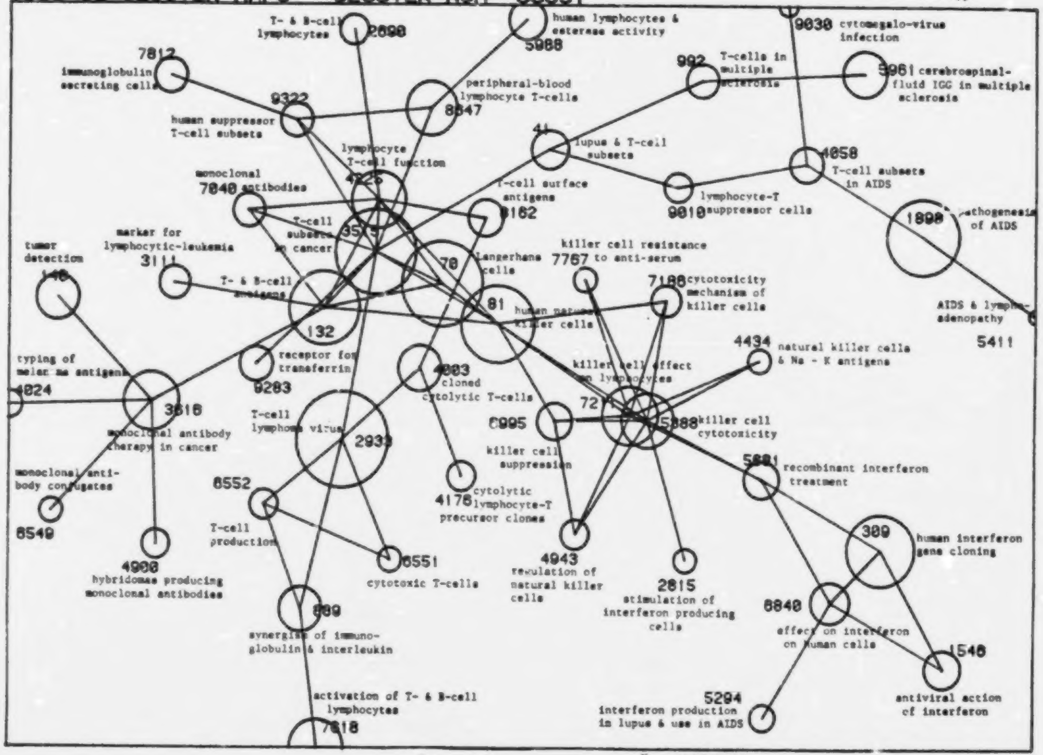
1983 C2 CLUSTER MAPS CLUSTER NUM 00031

T-cell and B-cell subsets in immunotherapy


T- & B-cell lymphocytes 2090
5988 human lymphocytes & esterase activity
9030 cytomegalo-virus infection
7812 immunoglobulin secreting cells
902 T-cells in multiple sclerosis
5961 cerebrospinal-fluid IGG in multiple sclerosis
9322 human suppressor T-cell subsets
8547 peripheral-blood lymphocyte T-cells
41 lupus & T-cell subsets
4058 T-cell subsets in AIDS
lymphocyte T-cell function 4225
monoclonal 7040 antibodies
T-cell surface antigens 8162
9010 lymphocyte-T suppressor cells
1890 pathogenesis of AIDS
T-cell subsets in cancer 3575
marker for lymphocytic-leukemia 3111
tumor detection 140
T- & B-cell antigens 132
78 Langerhans cells
7767 killer cell resistance to anti-serum
7180 cytotoxicity mechanism of killer cells
81 human natural killer cells
AIDS & lympho-adenopathy 5411
4434 natural killer cells & Na - K antigens
typing of melan ma antigens
4024
3816 monoclonal antibody therapy in cancer
9283 receptor for transferrin
4003 cloned cytolytic T-cells
killer cell effect on lymphocytes 721
5388 killer cell cytotoxicity
6995 killer cell suppression
T-cell lymphoma virus 2933
monoclonal anti-body conjugates 6549
8552 T-cell production
4178 cytolytic lymphocyte-T precursor clones
5081 recombinant interferon treatment
309 human interferon gene cloning
4900 hybridomas producing monoclonal antibodies
6551 cytotoxic T-cells
4943 regulation of natural killer cells
2815 stimulation of interferon producing cells
6840 effect on interferon on human cells
889 synergism of immuno-globulin & interleukin
1548 antiviral action of interferon
activation of T- & B-cell lymphocytes 7818
5294 interferon production in lupus & use in AIDS

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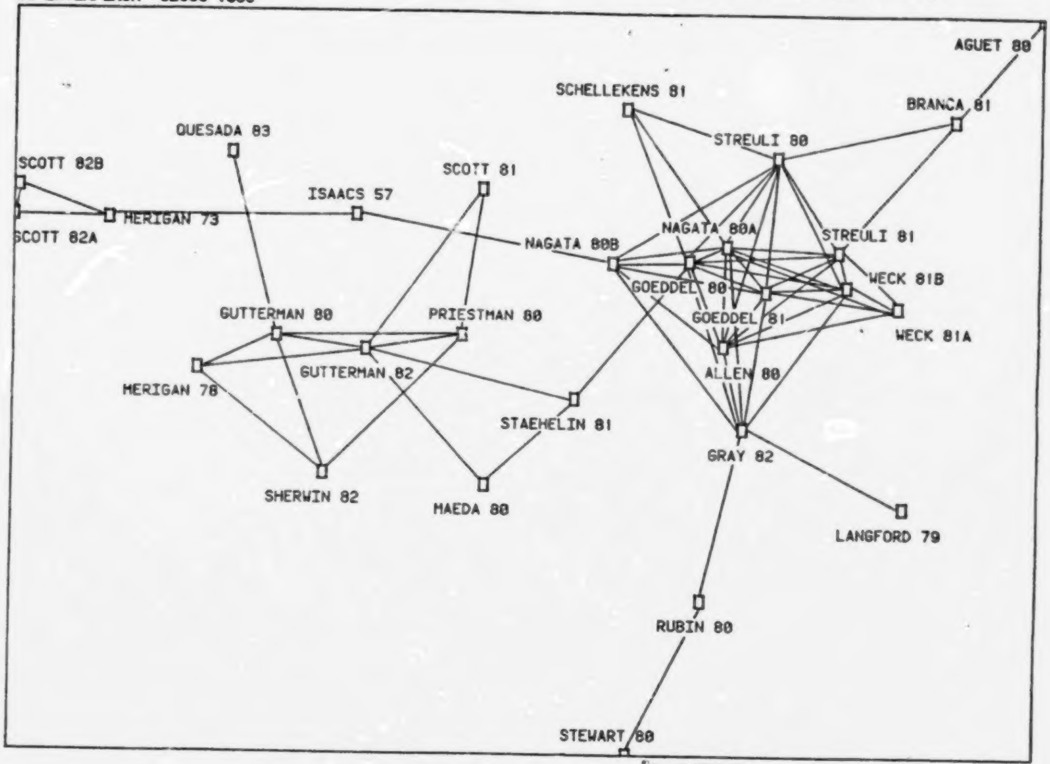
CI INTERFERON CL389 1983

human interferon gene cloning


AGUET 80
SCHELLEKENS 81
BRANCA 81
QUESADA 83
STREULI 80
SCOTT 82B
SCOTT 81
ISAACS 57
HERIGAN 73
SCOTT 82A
NAGATA 80B
NAGATA 80A
STREULI 81
WECK 81B
GOEDDEL 80
GOEDDEL 81
WECK 81A
GUTTERMAN 80
PRIESTMAN 80
GUTTERMAN 82
HERIGAN 78
ALLEN 80
STAEHELIN 81
GRAY 82
SHERWIN 82
MAEDA 80
LANGFORD 79
RUBIN 80
STEWART 80

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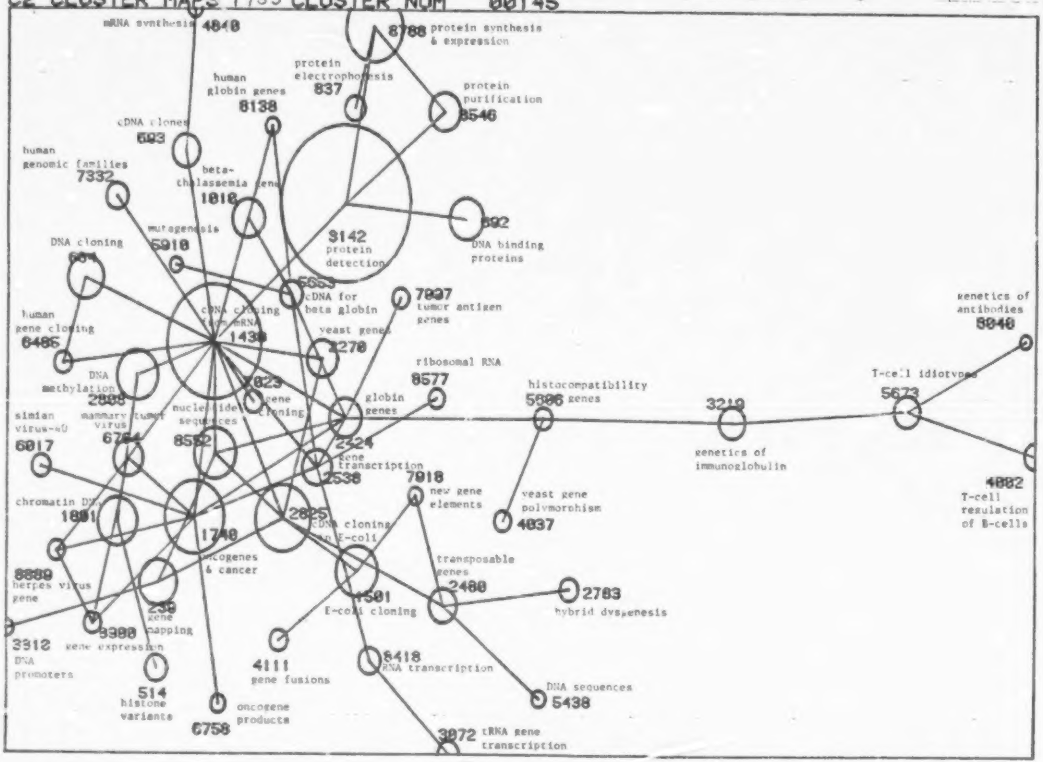
C2 CLUSTER MAPS 1983 CLUSTER NUM 00145 genetic engineering


mRNA synthesis 4840
8788 protein synthesis & expression
protein electrophoresis 837
human globin genes 8138
protein purification 8546
cDNA clones 593
human genomic families 7332
beta-thalassemia gene 1818
3142 protein detection
692 DNA binding proteins
mutagenesis 5918
DNA cloning 684
5663 cDNA for beta globin
7037 tumor antigen genes
human gene cloning 6485
cDNA cloning from mRNA 1438
yeast genes 2270
genetics of antibodies 8848
ribosomal RNA 8577
DNA methylation 2888
1823 gene cloning
globin genes 2424
histocompatibility 5886 genes
3219 genetics of immunoglobulin
T-cell idiotypes 5673
simian virus-40 6817
mammary tumor virus 6764
nucleotide sequences 8552
gene transcription 2538
7918 new gene elements
yeast gene polymorphism 4837
4882 T-cell regulation of B-cells
chromatin DNA 1881
1748 oncogenes & cancer
2825 cDNA cloning in E-coli
8889 herpes virus gene
239 gene mapping
4581 E-coli cloning
transposable genes 2488
2783 hybrid dysgenesis
3312 DNA promoters
3380 gene expression
4111 gene fusions
8418 RNA transcription
514 histone variants
6758 oncogene products
DNA sequences 5438
3872 tRNA gene transcription

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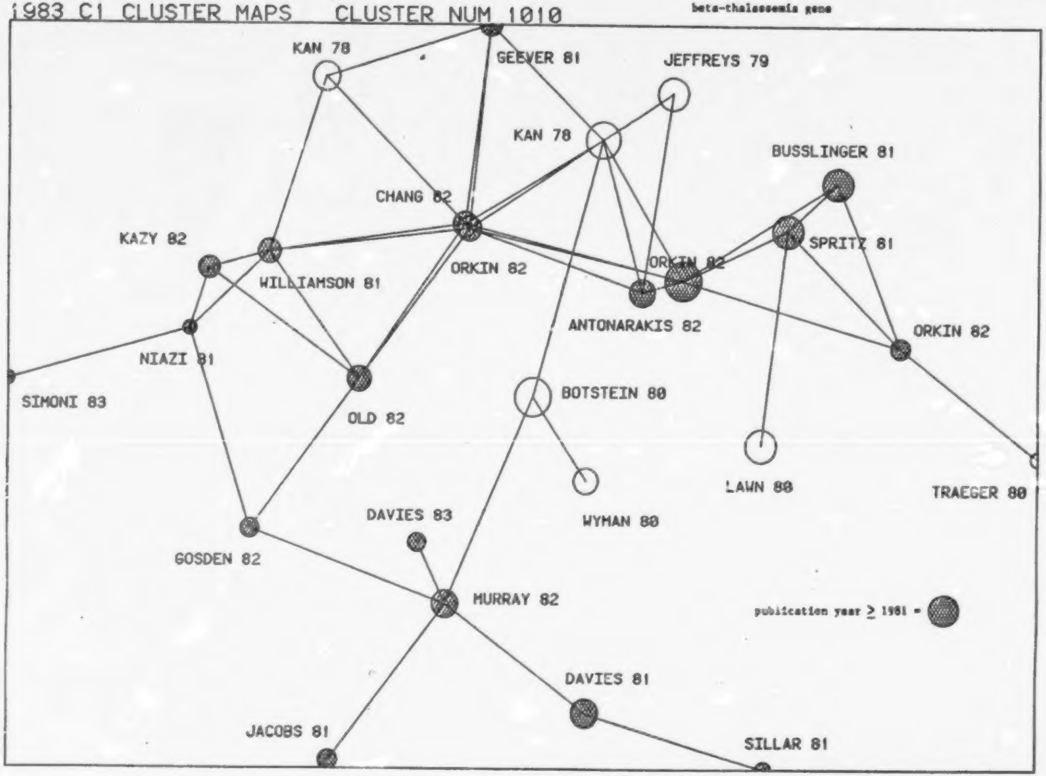
1983 C1 CLUSTER MAPS CLUSTER NUM 1010

beta-thalassemia gene


KAN 78
GEEVER 81
JEFFREYS 79
KAN 78
BUSSLINGER 81
CHANG 82
SPRITZ 81
KAZY 82
ORKIN 82
WILLIAMSON 81
ORKIN 82
ANTONARAKIS 82
ORKIN 82
NIAZI 81
SIMONI 83
OLD 82
BOTSTEIN 80
LAWN 80
TRAEGER 80
WYMAN 80
DAVIES 83
GOSDEN 82
MURRAY 82
publication year ≥ 1981 = ●
DAVIES 81
JACOBS 81
SILLAR 81

351

Figure 3

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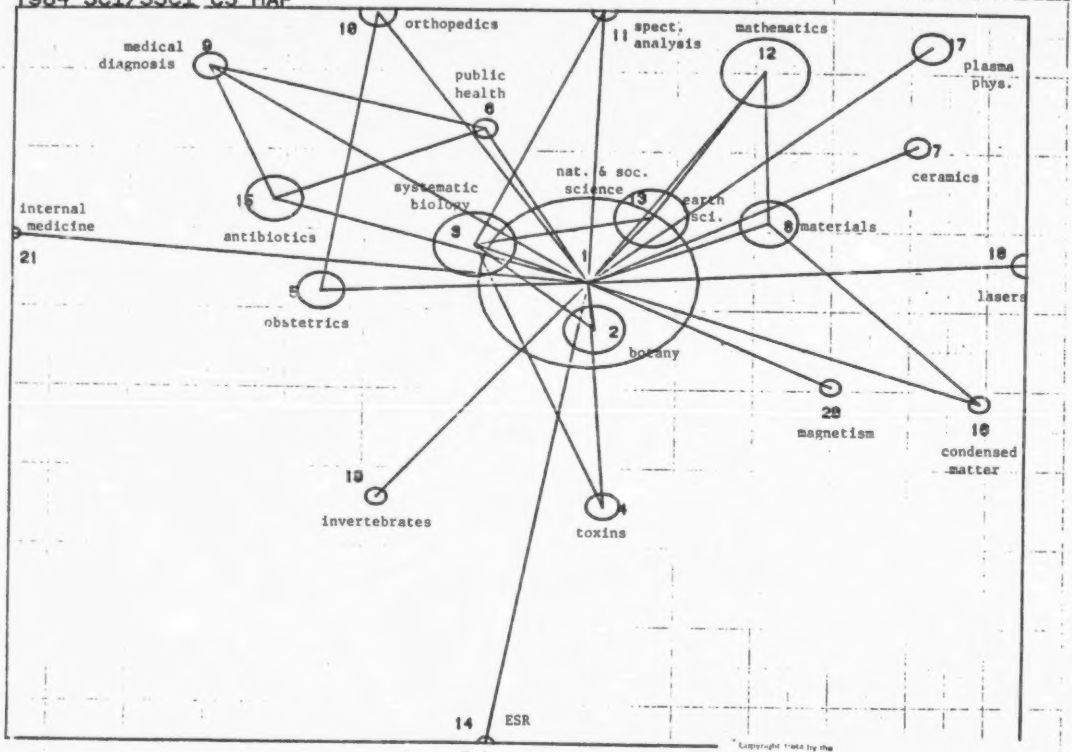
Global Map

1984 SCI/SSCI C5 MAP

②



10 orthopedics
11 spect. analysis
mathematics 12
17 plasma phys.
medical diagnosis 9
public health 6
7 ceramics
nat. & soc. science
systematic biology
13 earth sci.
internal medicine 21
15 antibiotics
3
8 materials
1
18 lasers
5 obstetrics
2 botany
20 magnetism
16 condensed matter
19 invertebrates
4 toxins
14 ESR

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352

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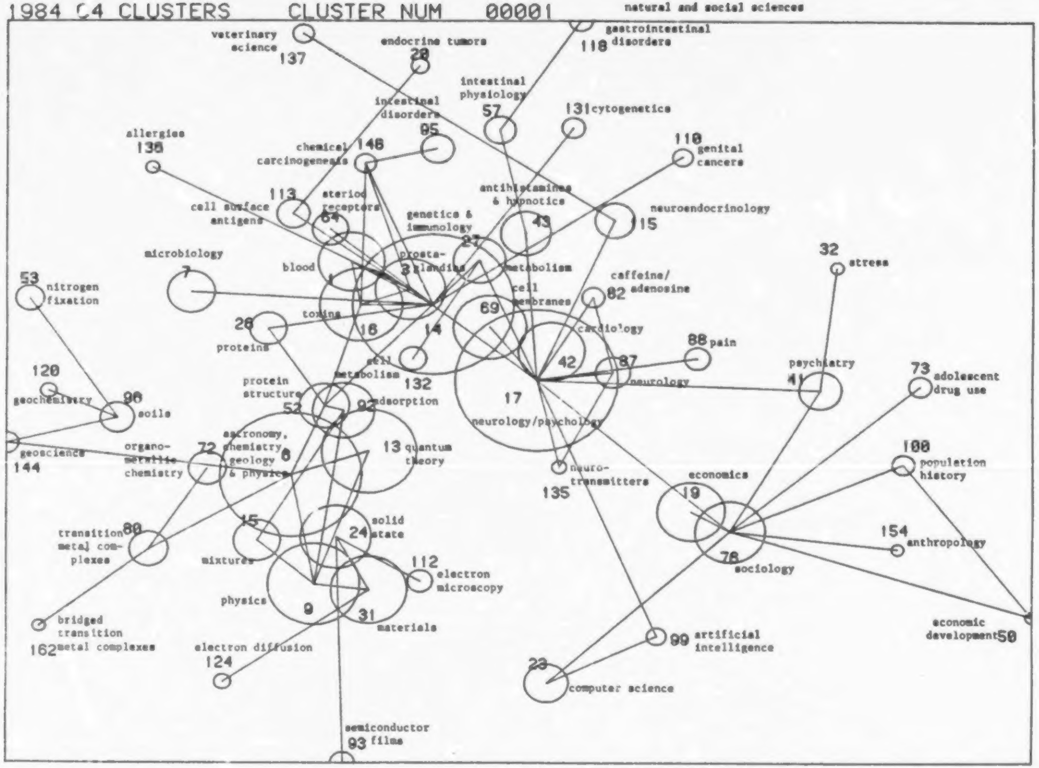
1984 C4 CLUSTERS CLUSTER NUM 00001 natural and social sciences


137 veterinary science
28 endocrine tumors
118 gastrointestinal disorders
57 intestinal physiology
95 intestinal disorders
131 cytogenetics
138 allergies
146 chemical carcinogenesis
110 genital cancer
113 cell surface antigens
64 steriod receptors
43 antihistamines & hypnotics
115 neuroendocrinology
27 genetics & immunology
3 prosta-glandins
metabolism
blood
1
7 microbiology
53 nitrogen fixation
32 stress
82 caffeine/adenosine
69 cell membranes
26 proteins
toxins
16
14
cardiology
88 pain
42
87 neurology
41 psychiatry
73 adolescent drug use
132 cell metabolism
120 geochemistry
96 soils
52 protein structure
92 adsorption
17 neurology/psychology
144 geoscience
72 organo-metallic chemistry
6 astronomy, chemistry, geology & physics
13 quantum theory
135 neuro-transmitters
100 population history
19 economics
80 transition metal complexes
15 mixtures
24 solid state
154 anthropology
78 sociology
112 electron microscopy
physics
9
31 materials
162 bridged transition metal complexes
124 electron diffusion
99 artificial intelligence
50 economic development
23 computer science
93 semiconductor films

353

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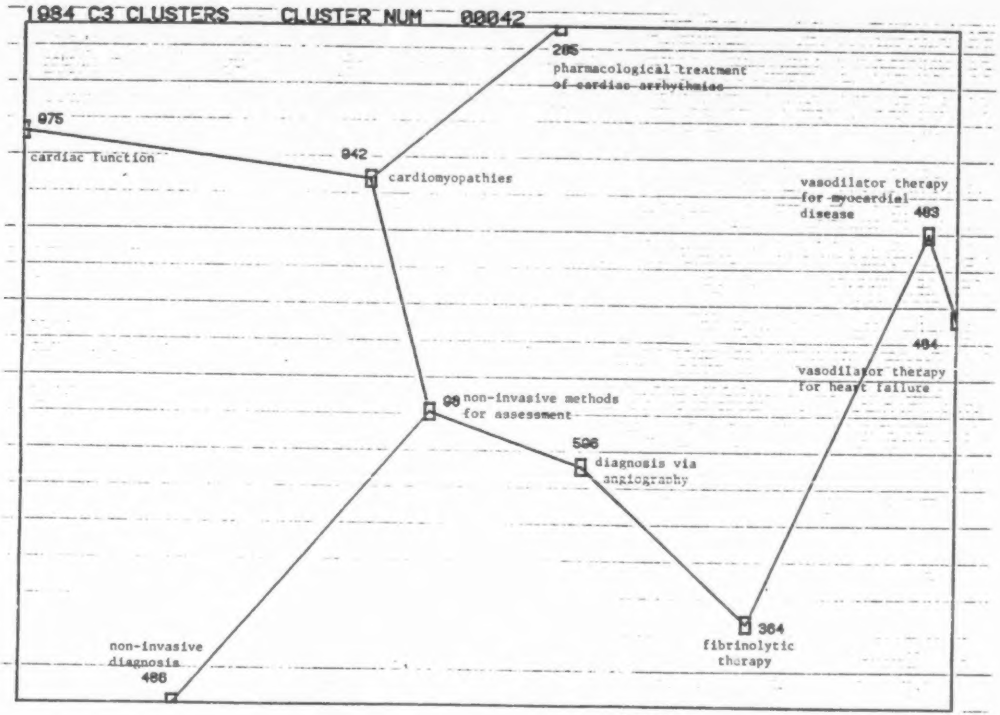
1984 C3 CLUSTERS CLUSTER NUM 00042
205
pharmacological treatment of cardiac arrhythmias
975
cardiac function
942
cardiomyopathies
vasodilator therapy for myocardial disease
403
404
vasodilator therapy for heart failure
98 non-invasive methods for assessment
596
diagnosis via angiography
364
fibrinolytic therapy
non-invasive diagnosis
486

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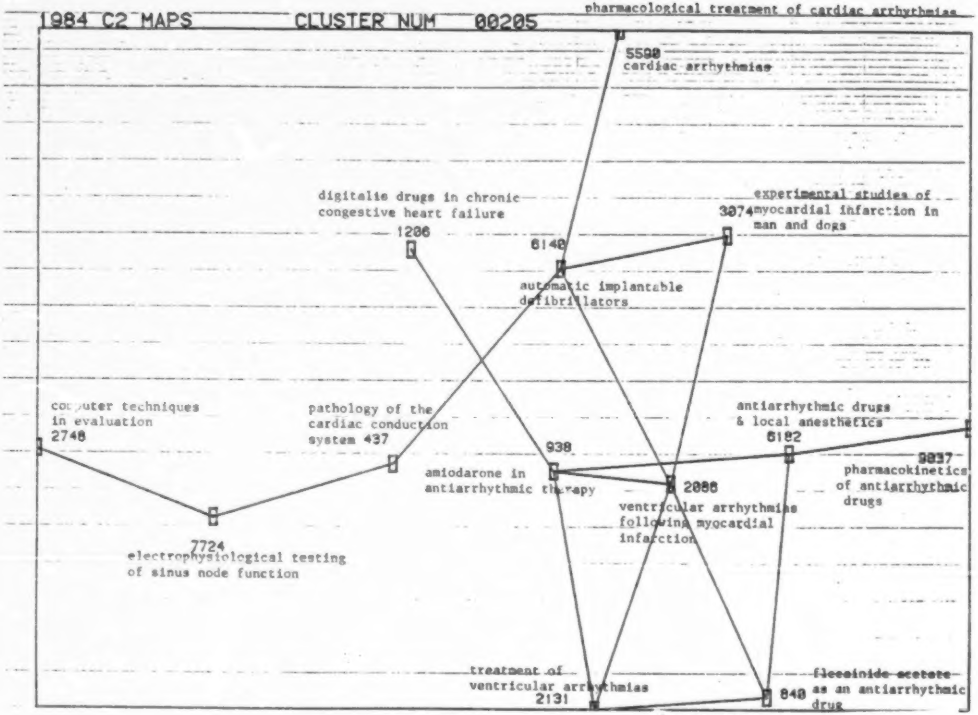
1984 C2 MAPS
CLUSTER NUM 00205
pharmacological treatment of cardiac arrhythmias
5590
cardiac arrhythmias
digitalis drugs in chronic congestive heart failure
1206
6140
automatic implantable defibrillators
3074
experimental studies of myocardial infarction in man and dogs
computer techniques in evaluation
2748
pathology of the cardiac conduction system 437
938
amiodarone in antiarrhythmic therapy
2086
ventricular arrhythmias following myocardial infarction
antiarrhythmic drugs & local anesthetics
6182
9837
pharmacokinetics of antiarrhythmic drugs
7724
electrophysiological testing of sinus node function
treatment of ventricular arrhythmias
2131
840
flecainide acetate as an antiarrhythmic drug

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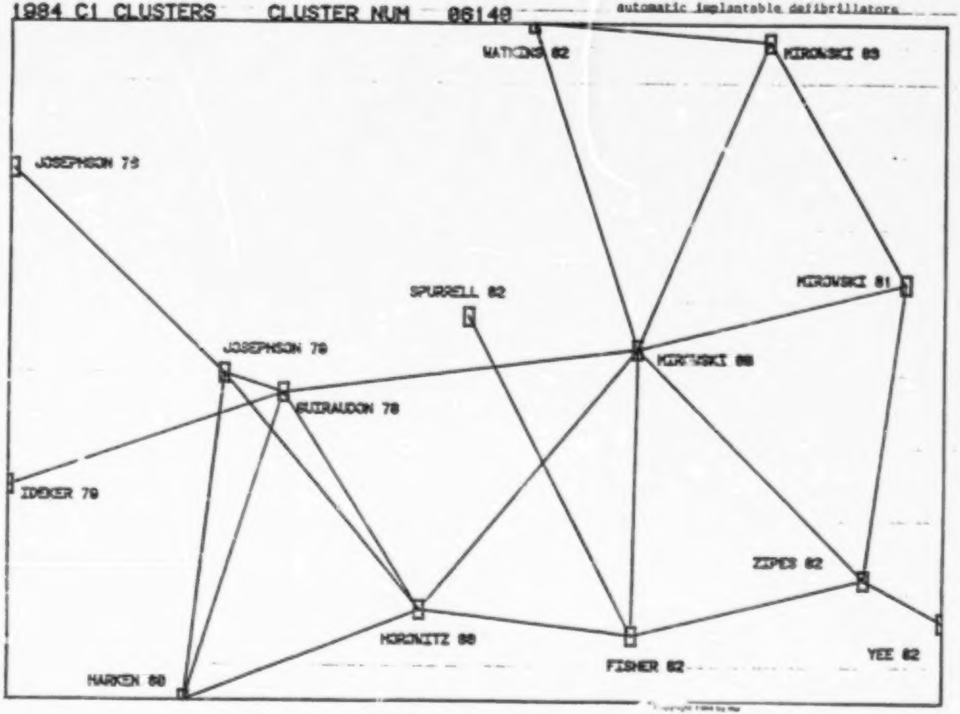
1984 C1 CLUSTERS CLUSTER NUM 06140 automatic implantable defibrillators


WATKINS 82
MIROWSKI 83
JOSEPHSON 78
SPURRELL 82
MIROWSKI 81
JOSEPHSON 78
MIROWSKI 80
GUIRAUDON 78
IDEKER 79
ZIPES 82
HOROWITZ 80
FISHER 82
YEE 82
HARKEN 80

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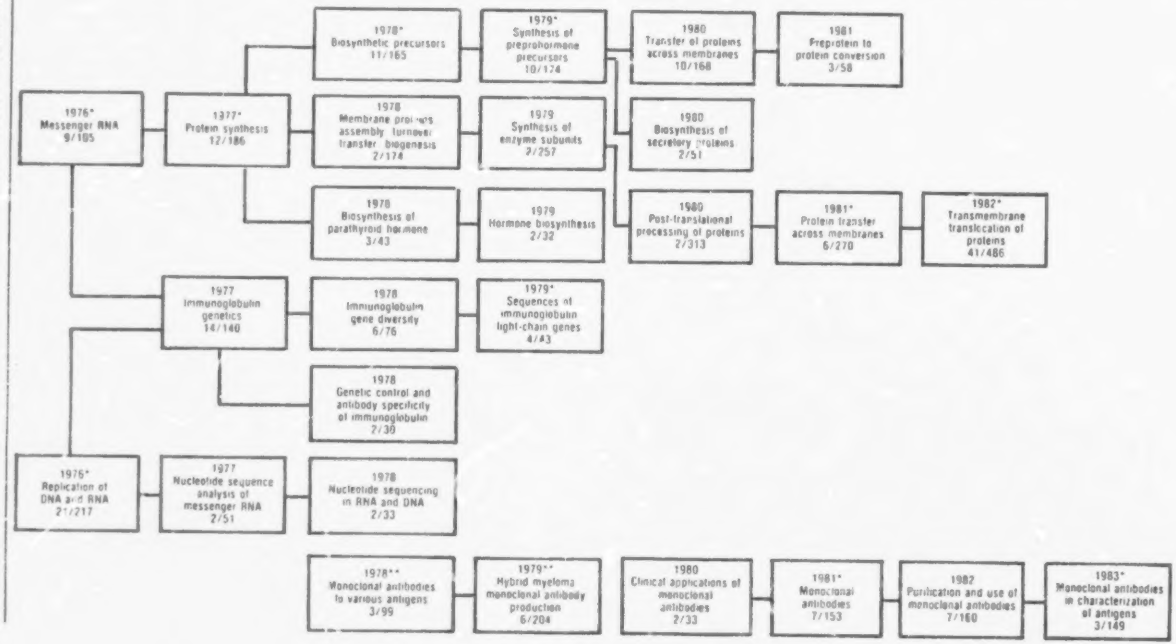
356

Figure 5: Historiograph tracing research leading to the monoclonal antibody technique. Numbers at the bottom of each box refer to the number of cited/citing papers for each research front. One asterisk (*) indicates research fronts in which Milstein is an author of a core paper; two asterisks (**) indicate research fronts in which both Milstein and Köhler are authors of core papers.


1976*
Messenger RNA
9/105
1977*
Protein synthesis
12/186
1978*
Biosynthetic precursors
11/165
1979*
Synthesis of
preprohormone
precursors
10/174
1980
Transfer of proteins
across membranes
10/168
1981
Preprotein to
protein conversion
3/58
1978
Membrane proteins
assembly turnover
transfer biogenesis
2/174
1979
Synthesis of
enzyme subunits
2/257
1980
Biosynthesis of
secretory proteins
2/51
1978
Biosynthesis of
parathyroid hormone
3/43
1979
Hormone biosynthesis
2/32
1980
Post-translational
processing of proteins
2/313
1981*
Protein transfer
across membranes
6/270
1982*
Transmembrane
translocation of
proteins
41/486
1977
Immunoglobulin
genetics
14/140
1978
Immunoglobulin
gene diversity
6/76
1979*
Sequences of
immunoglobulin
light-chain genes
4/43
1978
Genetic control and
antibody specificity
of immunoglobulin
2/30
1976*
Replication of
DNA and RNA
21/217
1977
Nucleotide sequence
analysis of
messenger RNA
2/51
1978
Nucleotide sequencing
in RNA and DNA
2/33
1978**
Monoclonal antibodies
to various antigens
3/99
1979**
Hybrid myeloma
monoclonal antibody
production
6/204
1980
Clinical applications of
monoclonal
antibodies
2/33
1981*
Monoclonal
antibodies
7/153
1982
Purification and use of
monoclonal antibodies
7/160
1983*
Monoclonal antibodies
in characterization
of antigens
3/149

Mapping science in the Third World

Dr E. Garfield contributes importantly to the world information order debate through an analysis of Third World Science employing the cluster mapping methodology developed by the Institute for Scientific Information

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President
Institute for Scientific Information
3501 Market Street
University City Science Center
Philadelphia, PA 19104
USA

This article is an edited version of the 1982 Annual Magnus Pyke Science Policy Foundation Lecture.

Eugene Garfield PhD *is a pioneer in the field of information science. He is founder and president of the Institute for Scientific Information (ISI), which produces a broad spectrum of information services and systems. The inventor of 'Current Contents' and the 'Science Citation Index', he received his BS in chemistry from Columbia University, and returned there for a Master's degree in library science. He was awarded his PhD in structural linguistics at the University of Pennsylvania.*

Cartoons by Richard Willson

Keywords: information science, Third World science, cluster mapping

I am honoured to present the 1982 Annual Magnus Pyke Science Policy Foundation Lecture. It gives me an opportunity to expand on a topic that has interested me for many years – science in the developing countries. Most scientists in the West are not aware of research going on in Peru, Ghana, Singapore, or other countries collectively called the Third World. And what they do know about Third World science is dominated by the research of one or two Third World 'research superpowers' – India or Argentina, for example.

An analysis of Third World science is especially relevant today. The New Information Order is a much debated issue in international forums, such as UNESCO.[1-4] Third World countries have charged that news from their part of the world is under-reported in the international press. And what little news of the developing countries is reported allegedly gives a biased and sensational view of the Third World – earthquakes and revolutions, eruptions and government overthrows.

Only four news agencies currently account for more than 90% of all foreign news transmitted around the world.[5] All are in the West – Reuters, Agence France-Presse, Associated Press, and United Press International. In the light of the Western monopoly on international information, Third World nations sometimes feel powerless to control and contribute to what is said about them. As a result, a New Information Order has been proposed, one in which Third World news is given increased and more balanced coverage.

This is a controversial issue, but perhaps I can demonstrate statistically why the Third World feels the way it does. The representation of Third World research in international scientific journals is a neglected aspect of the New Information Order debate. How many research articles from Third World countries are published in journals from the developed countries? And what impact does Third World research have on the international scientific community? The term 'impact' is vague without a definition. Here, impact is defined in terms of citations. I realize that this, too, is controversial.

Citation method

When a scientist cites a given article, he or she indicates that the article was somehow relevant to the research performed. The citing author calls attention to some useful place of information included in that article – a method, statistic, result, or whatever. And when an article is cited many times, it can be considered to have had a significant impact on the conduct of scientific research. I do not discuss here the vast literature which supports the validity of this premise.

If we take into account only the number of articles an entire nation's scientists authored, we get an idea of their level of productivity. But when we also consider the number of citations these articles received, we have a measure of their utility or impact. The purpose here is to document the level and impact of Third World research in the world's scientific journals. Perhaps it will serve as a point of departure for a wider discussion on the need for a New *Scientific* Information Order.

The Institute for Scientific Information® (ISI®), of which I am founder and chairman, offers a unique viewpoint on Third World science. Every year, we monitor several thousand scientific journals in our *Science Citation Index*® (*SCI*®) database. We record bibliographic information on every item published in these journals. Last year, we processed 540 000 research articles, reviews, notes, letters, editorials, and other scientific communications. Similar data are available from our *Social Sciences Citation Index*® (*SSCI*®) database, and the *Arts & Humanities Citation Index*® (A&HCI®) data. Here, I restrict myself to the *SCI*.

SCI source index

We recorded the names and addresses of about 1.5 million authors, the title, issue, and pages of 3 000 journals they published in, and about nine million references they cited. This information allows us to tabulate the number of articles written by Third World researchers that were published in the journals we covered. That is, we can identify all articles with first authors who list a Third World country as their address.

SCI citation index

Our records also let us calculate the number of citations these Third World articles received. We can tell who the citing author is, and the journal, volume, page, and year of the citing article.

Third World study

For convenience, we used the *Encyclopedia of the Third World*[6] to define which countries are considered as member states of the Third World. In 1982, the encyclopedia listed 122 developing nations. These countries met a variety of economic requirements in order to be considered developing nations. In addition, they were non-aligned with the USA or USSR.

As a source year for this study, we decided to use the 1973 *SCI* database. This gives a base year to compare the performance of Third World researchers in the future. In fact, we are now processing both the 1978 and 1981 *SCI* files to update this study. Of course, it takes an immense effort to produce and collate these data. But in a short time, we shall be able to chart the growth or decline of Third World science from 1973 to 1978 and 1981.

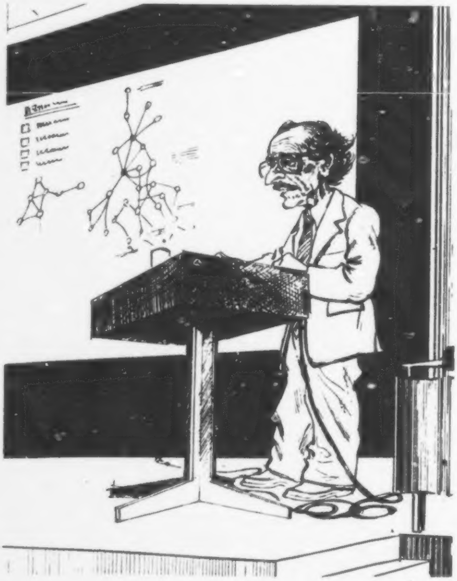
Also, using 1973 as a base year gives us enough time to measure the impact of Third World research on the international scientific community. That is, we can determine the number of citations these Third World articles received over a period of several years. The data are taken from six annual *SCI* files, covering 1973–1978.

There are a few important caveats. This discussion of Third World science is limited to that fraction of the international scientific press covered in the 1973 *SCI*. In 1973, it was estimated that 50 000 science journals were published worldwide.[7] This figure is a gross distortion, and it is safe to say that over 90% of these publications are of minor significance. The *SCI* covered about 2 500 carefully selected journals that year.

Our studies indicate that the 'important' high-impact science journals actually amount to no more than 500 to 1 000 journals.[8,9] For example, the solid line in Figure 1 shows that only 80 journals are cited in 25% of all references processed in the 1981 *SCI*. In other words, only 2% of the 4 000 journals covered in the *SCI* that year account for 25% of all citations. And only 700 journals account for 52% of all citations. Thus, a small number of journals account for the majority of citations.

Further evidence of the concentration of scientific literature in a small group of journals is provided by the dotted line in Figure 1 which shows that 20% of all 1981 articles were published in only 60 journals. And 400 journals account for 50% of all published articles. Again, a small group of journals predominates in the scientific literature.

I am confident that the journals *ISI* covers represent the major channels of international scientific communication. It should be borne in mind that we are assessing the level and impact of Third World research in



international scientific journals – it is not intended to provide an inventory of Third World scientific output in every journal from every country of the world.

Table 1 shows how the articles in the 1973 *SCI* were distributed by country of publication. About 353 000 articles were indexed in the *SCI* that year, and these articles received 2 million citations from 1973 to 1978. US journals alone published 48% of all *SCI* articles in 1973, and US published articles account for 60% of the 2 million citations in the 1973–78 *SCI* files. The average article published in a US journal received about seven citations over that six-year period.

If we add together the publication output of the USA, the UK and Commonwealth countries, Western Europe, Japan and Scandinavia, we see that these 'First World' countries account for 84% of 1973 articles, and 89% of 1973–78 citations. Clearly, the *SCI* database reflects the dominance of First World scientific publications. Western journals control the flow of international scientific communication almost as much as Western news agencies 'monopolize' international news. This is not a judgement, but simply a statement of fact.

Table 2 shows how the 1973 articles were distributed by nationality of the first author. Articles written by first authors listing a US institution as their address account for the largest portion – 43%. These articles also account for the lion's share of citations – more than half. And if we add together articles authored in the USA, UK and Commonwealth countries, Western Europe, Japan and Scandinavia, they account for 83% of 1973 articles, and 93% of 1973–78 citations. The data indicate that researchers from First World countries are the major participants in international science, at least as it is reflected in the *SCI*.

Position of India

Table 3 identifies the top 25 countries in terms of the number of articles written by their authors, only two Third World countries appear – India and Argentina. India's rank is significant. It is considered to rank third in the world in the number of researchers, behind the USA and USSR.[7] But it ranks eighth when we consider the number of articles its researchers authored in the international journals discussed here.

We can expect India to dominate the Third World when we rank

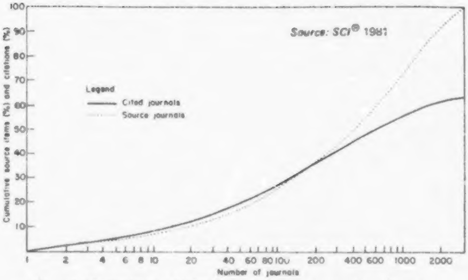


Figure 1. Distribution of published items and citations among science journals

Table 1. Publishing countries represented in the 1973 *SCI*® database

Geographical region	% of 1973 SCI articles	% of 1973–78 SCI citations	Impact
USA	48	60	6.9
UK/Commonwealth	16	16	5.5
Western Europe	15	10	3.4
USSR	6	2	1.4
Eastern Europe	3	1	1.9
Japan	3	2	2.9
Scandinavia	2	2	7.6
All Third World	2	–	.8
All others	1	–	1.4

Table 2. Locations of authors in the 1973 *SCI*® database

Geographical region	% of 1973 SCI articles	% of 1973–78 SCI citations	Impact
USA	43	54	6.9
Western Europe	17	14	4.6
UK/Commonwealth	16	17	5.9
USSR	7	2	1.6
All Third World	5	2	2.3
Eastern Europe	4	2	2.5
Japan	4	3	4.1
Scandinavia	3	4	7.4
All Others	2	2	5.2

Table 3. Top 25 countries, ranked by number of articles their authors produced. Asterisks indicate Third World countries

Country	Articles	Citations	Impact	Cited articles	Uncited articles	% citedness
USA	151 939	1 047 854	6.9	97 852	54 087	64
UK	32 189	202 600	6.3	21 387	10 802	66
USSR	24 715	40 455	1.6	11 159	13 556	45
FRG	20 137	93 233	4.6	12 981	7 156	64
France	17 707	72 912	4.1	11 069	6 638	62
Japan	15 569	64 160	4.1	10 161	5 408	65
Canada	15 362	86 654	5.6	10 688	4 674	70
*India	7 888	15 515	2.0	4 568	3 320	58

continued on page 115

developing countries by the number of articles their researchers authored (Table 4). In fact, Indian researchers alone authored half the 16 000 articles from the Third World. Out of 93 Third World author countries in the 1973 *SCI* database, 30 developing countries authored 50 or more articles. These 30 countries accounted for 95% of all Third World articles. Clearly, India is the research 'superpower' of the Third World. Argentina is a distant second, accounting for 10%, or a fifth of India's output.

Ranking countries by the number of authored articles has its disadvantages. Small developing *and* developed countries producing high-quality science would tend not to appear. Their overall output is dwarfed by that of the large superpowers. But when we rank all countries by the *impact* of their authored articles, we see some interesting results.

Impact of Third World articles

Thirty countries averaged four or more citations to their authored articles from 1973–78 (Table 5). Twelve are developing countires, and they are indicated by asterisks. Three developing countries rank among the top ten in the world on impact – Liberia, Jamaica, and Thailand. However, we should note that Liberia's high impact is based on only seven authored articles. It would be difficult to derive much significance from so small a sample.

Articles authored in Liberia, Jamaica, and Thailand together averaged seven citations over a six-year period. This comes close to matching the impact of those Scandinavian countries in the top ten – they averaged eight citations. Liberian, Jamaican, and Thai articles, together or singly, had a greater impact than US articles. But India is conspicuous by its absence.

If we examine the impact of Third World articles only (Table 6), we see that 27 developing countries had an impact of 2.5 or greater. But India still does not appear, even though it dominated the Third World in authored articles. The average article from Indica was cited twice over six years. Argentina, which accounted for only a fifth of India's output, had a higher impact – 2.7.

Languages

Third World articles were written in ten different languages. More than

Table 3. continued

Australia	6 985	38 342	5.5	4 798	2 187	69
Italy	6 012	22 276	3.7	3 448	2 564	57
Sweden	4 989	42 078	8.4	3 748	1 241	75
Switzerland	4 483	29 078	6.5	2 940	1 543	66
Netherlands	4 114	28 415	6.9	2 971	1 143	72
Czechoslovakia	3 497	9 859	2.8	2 207	1 290	63
Israel	3 199	20 788	6.5	2 274	925	71
Poland	2 918	7 072	2.4	1 676	1 242	57
Belgium	2 675	12 532	4.7	1 772	903	66
Denmark	2 398	18 460	7.7	1 745	653	73
GDR	2 344	6 401	2.7	1 463	881	62
Hungary	2 209	5 025	2.3	1 068	1 141	48
Norway	1 850	11 200	6.0	1 292	558	70
Austria	1 753	5 205	3.0	1 106	647	63
South Africa	1 676	5 182	3.1	992	684	59
Finland	1 669	9 467	5.7	1 162	507	70
*Argentina	1 526	4 110	2.7	655	871	43

Source of data on articles: 1973 *SCI*®
Source of data on citations: 1973–78 *SCI*®

Table 4. Third World countries whose authors produced 50 or more articles, ranked by the number of articles

Country	Articles	Citations	Impact	Cited articles	Uncited articles	% citedness
India	7 888	15 515	2.0	4 568	3 320	58
Argentina	1 526	4 110	2.7	655	871	43
Brazil	812	2 355	2.9	401	411	49
Egypt	713	1 306	1.8	451	262	63
Venezuela	589	702	1.2	127	462	22
Chile	565	1 228	2.2	227	338	40
Mexico	535	1 652	3.1	258	277	48
Nigeria	354	866	2.4	229	125	65
Iran	196	444	2.3	107	89	55
Turkey	184	405	2.2	110	74	60
Malaysia	154	361	2.3	96	58	62
Lebanon	153	401	2.6	90	63	59
Singapore	139	305	2.2	88	51	63
Thailand	138	970	7.0	96	42	70
Uganda	132	587	4.4	93	39	70
Kenya	127	595	4.7	89	38	70
Pakistan	111	197	1.8	60	51	54
Zimbabwe	87	236	2.7	53	34	61
Ghana	79	140	1.8	44	35	56
Jamaica	77	545	7.1	63	14	82
Philippines	61	190	3.1	38	23	62
Peru	59	125	2.1	25	34	42
Tanzania	58	159	2.7	39	19	67
Sri Lanka	58	123	2.1	43	15	74
Sudan	57	161	2.8	36	21	63
Uruguay	57	121	2.1	24	33	42
Algeria	56	82	1.5	28	28	50
Colombia	54	159	2.9	35	19	65
Iraq	54	248	4.6	31	23	57
Ethiopia	50	247	4.9	40	10	80
Totals	15 123	34 535	2.3	8 244	6 879	54
All other Third World nations	755	1 630	2.2	410	345	54
Grand totals	15 878	36 165	2.3	8 654	7 224	55

Source of data on articles: 1973 *SCI*®
Source of data on citations: 1973–78 *SCI*®

85% of the 16 000 Third World articles are in English. Spanish is a distant second, accounting for 11%. Also, Third World articles had the highest impact (measured as total articles published divided by number of citations) when they were authored in English (impact of 2.6). German langauge articles had the next highest impact (2.0), followed by French (1.4) and Italian (1.0).

Clearly, English is the *lingua franca* of Third World science. This is even more true in recent years. For example, the 1978 *SCI* database included 22 000 articles from the Third World; 92% of these were in English. And English language articles again accounted for 92% of the 27 000 Third World articles in the 1981 *SCI* file.

In fact, the same pattern holds true for *all* the author countries in the *SCI* files, developed or developing. More than 80% of the 350 000 articles in the 1973 *SCI* were in English. And English language articles had the highest impact – 6.3. In the 1978 *SCI*, 87% of the 530 000 articles were in English. And English language articles accounted for 88% percent of the 605 000 articles in the 1981 *SCI* file.

This reminds me of a controversy involving France that developed in 1976. I presented data showing that most French research had a low impact relative to many other countries.[10] And this was even worse when they published their results in French. I showed that the best articles from France, with a significantly higher impact, were written in English.[11] This so offended the francophile establishment that I was accused of linguistic imperialism.[12] In fact, Michel Debré, a former Prime Minister of France, suggested that my data might threaten "the existence and permanence of the French nation" if it encouraged French scientists to publish in English![13]

But a recent survey of PASCAL, a French documentation system, yielded some ironic results. In 1979, PASCAL indexed 155 000 articles in its life sciences file. Of these, 70% were in English. French amounted to only 12%, and German to 7%.[14] Unfortunately, PASCAL does not record citations, so we have no measure of the relative impact of these articles. Even a database that "is supposed to be strongly biased in favor of European languages"[14] shows that English *is* the *lingua franca* of international science.

So far, we have looked at 350 000

Table 5. Countries with impact of 4.0 or greater for articles their authors produced, ranked by impact. Asterisks indicate Third World countries

Country	Impact	Articles	Citations	Cited articles	Uncited articles	% citedness
Bermuda	11.5	2	23	2	–	100
*Liberia	8.7	7	61	7	–	100
Sweden	8.4	4 989	42 078	3 748	1 241	75
Denmark	7.7	2 398	18 460	1 745	653	73
Antilles	7.5	4	30	4	–	100
*Jamaica	7.0	77	545	63	14	82
*Thailand	7.0	138	970	96	42	70
Netherlands	6.9	4 114	28 415	2 971	1 143	72
USA	6.9	151 939	1 047 854	97 852	54 087	64
Israel	6.5	3 199	20 788	2 274	925	71
Switzerland	6.5	4 483	29 078	2 940	1 543	66
UK	6.3	32 189	202 600	21 387	10 802	66
Norway	6.0	1 850	11 200	1 292	558	70
Finland	5.7	1 669	9 467	1 162	507	70
Canada	5.6	15 362	86 654	10 688	4 674	70
Australia	5.5	6 985	38 342	4 798	2 187	69
*Guatemala	5.3	18	96	12	6	67
Northern Ireland	5.2	539	2 791	359	180	67
*Panama	5.2	16	83	13	3	81
*Ethiopia	4.9	50	247	40	10	80
Belgium	4.7	2 675	12 532	1 772	903	66
*Iraq	4.7	54	248	31	23	58
*Kenya	4.7	127	595	89	38	70
FRG	4.6	20 137	93 233	12 981	7 156	64
*Uganda	4.4	132	587	93	39	70
*Zambia	4.4	41	179	24	17	58
Japan	4.1	15 569	64 160	10 161	5 408	65
*Cameroon	4.1	16	65	9	7	56
France	4.1	17 707	72 [illegible]12	11 069	6 638	62
*Congo Peop Rep	4.0	8	32	5	3	62

Source of data on articles: 1973 *SCI*®

Source of data on citations: 1973–78 *SCI*®

Table 6. Third World countries with impact of 2.5 or greater for articles their authors produced, ranked by impact

Country	Impact	Articles	Citations	Cited articles	Uncited articles	% citedness
Liberia	8.7	7	61	7	–	100
Jamaica	7.1	77	545	63	14	82
Thailand	7.0	138	970	96	42	70
Guatemala	5.3	18	96	12	6	67
Panama	5.2	16	83	13	3	81
Ethiopia	4.9	50	247	40	10	80
Iraq	4.7	54	248	31	23	58
Kenya	4.7	127	595	89	38	70
Uganda	4.4	132	587	93	39	70
Zambia	4.4	41	179	24	17	58
Cameroon	4.1	16	65	9	7	56
Congo Peop Rep	4.0	8	32	5	3	62
Mexico	3.1	535	1 652	258	277	48
Philippines	3.1	61	190	38	23	62
Burundi	3.0	1	3	1	–	100
Lesotho	3.0	2	6	2	–	100
Brazil	2.9	812	2 355	401	411	49

continued on page 117

articles in the 1973 *SCI* file by the nationality of the first author. We identified about 16 000 articles as having been authored in 93 Third World countries. India was found to be the 'superpower' of Third World research, accounting for half of all articles from developing countries. But when impact was considered, Indian articles did not rank among the top 25 countries, developed or developing. But three Third World countries – Liberia, Jamaica, and Thailand – had a combined impact close to Scandinavia's, and greater than that of the USA. Also, English language articles dominated the total output of Third World authors, and they had the highest impact.

Nationality

We now consider the same articles by the nationality of the publisher. Fifteen Third World publishing countries are represented in the 1973 *SCI* database. We covered 52 of their journals. These journals published 5 500 of the 16 000 articles written by Third World authors. That is, 35% of all the articles by Third World authors were published in Third World journals.

Impact of Third World publishing countries

Again, India is the giant of Third World science. It published 60% of all articles in the Third World scientific press (Table 7). Argentina ranks second, accounting for 20%. The remaining 13 Third World publishing countries account for only 20%.

India and Argentina also rank among the top 25 scientific publishing countries in the world. India ranks 13th and Argentina 20th.

India also ranks first among Third World publishing countries when impact is taken into account (Table 8). The average article published in an Indian journal was cited once from 1973–78. Costa Rica has an equivalent impact, even though its output of published articles was much smaller than that of India. The publications of Peru, Kuwait, Philippines, and Colombia are not listed here because they were not cited from 1973–78.

Publishing opportunities

The USA published more Third World articles than any other country (Table 9). It published 3 700 Third World articles. This amounts to only 2% of all US published articles. But it

Table 6. continued

Colombia	2.9	54	159	35	19	65
Barbados	2.8	5	14	4	1	80
Ivory Coast	2.8	35	99	28	7	80
Saudi Arabia	2.8	13	37	8	5	61
Sudan	2.8	57	161	36	21	63
Argentina	2.7	1 526	4 110	655	871	43
Indonesia	2.7	23	62	10	13	43
Tanzania	2.7	58	159	39	19	67
Lebanon	2.6	153	401	90	63	59
Zimbabwe	2.6	87	236	53	34	61
Totals	**3.2**	**4 106**	**13 352**	**2 140**	**1 966**	**52**
All other Third World nations	**1.9**	**11 772**	**22 813**	**6 514**	**5 258**	**55**
Grand totals	**2.3**	**15 878**	**36 165**	**8 654**	**7 224**	**54**

Source of data on articles: 1973 *SCI*®
Source of data on citations: 1973–78 *SCI*®

Table 7. Third World publishing countries, ranked by the number of articles published in their journals

Publishing country	Articles	Citations	Impact	Cited articles	Uncited articles	% Citedness
India	3 486	3 815	1.1	1 657	1 829	48
Argentina	1 137	379	.3	157	980	14
Venezuela	473	93	.2	49	424	10
Chile	288	123	.4	61	227	21
Mexico	187	99	.5	42	145	22
Brazil	109	29	.3	15	94	14
Costa Rica	71	74	1.0	31	40	44
Lebanon	21	6	.3	5	16	24
Ghana	18	3	.2	3	15	17
Peru	16	0	–	0	16	–
Nigeria	13	5	.4	4	9	31
Kuwait	10	0	–	0	10	–
Thailand	7	1	.1	1	6	14
Philippines	5	0	–	0	5	–
Colombia	2	0	–	0	2	–
Totals	**5 843**	**4 627**	**.8**	**2 025**	**3 818**	**35**

Source of data on articles: 1973 *SCI*®
Source of data on citations: 1973–78 *SCI*®

Table 8. Third World publishing countries, ranked by impact of articles their journals published

Publishing country	Impact	Articles	Citations	Cited articles	Uncited articles	% Citedness
India	1.1	3 468	3 815	1 657	1 829	48
Costa Rica	1.0	71	74	31	40	44
Mexico	.5	187	99	42	145	22
Chile	.4	288	123	61	227	21
Nigeria	.4	13	5	4	9	31
Argentina	.3	1 137	379	157	980	14
Brazil	.3	109	29	15	94	14
Lebanon	.3	21	6	5	16	24
Ghana	.2	18	3	3	15	17
Venezuela	.2	473	93	49	424	10
Thailand	.1	7	1	1	6	14

Source of data on articles: 1973 *SCI*®
Source of data on citations: 1973–78 *SCI*®

Table 9. Countries that published 50 or more Third World articles, ranked by the number of articles. Asterisks indicate Third World countries

Publishing country	Articles	Citations	Impact	Third World countries
USA	3 755	13 706	3.6	75
*India	3 351	3 690	1.1	12
UK	2 542	8 401	3.3	68
*Argentina	1 069	348	.3	13
Netherlands	885	3 440	3.9	53
FRG	648	1 512	2.3	43
Switzerland	555	1 488	2.7	41
*Venezuela	431	53	.1	5
France	360	566	1.6	42
GDR	319	602	1.9	17
Japan	293	477	1.6	19
*Chile	283	123	.4	5
Italy	228	280	1.2	29
*Mexico	174	94	.5	7
Australia	134	267	2.0	20
Denmark	126	351	2.8	23
*Brazil	89	22	.2	2
Canada	86	152	1.8	20
Hungary	74	84	1.1	10
Austria	64	107	1.7	8
Czechoslovakia	54	94	1.7	9
*Costa Rica	50	58	1.2	12
Totals	**15 570**	**36 915**	**2.3**	
All others	**308**	**250**	**.8**	
Totals	**15 878**	**36 165**	**2.3**	

Source of data on articles: 1973 *SCI*®

Source of data on citations: 1973–78 *SCI*®

Table 10. Publishing countries with an impact of 1.5 or greater for Third World articles, ranked by impact

Publishing country	Impact	Articles	Citations	Third World countries
Netherlands	3.9	885	3 440	53
USA	3.6	3 755	13 706	75
UK	3.3	2 542	8 401	68
Ireland	3.0	1	3	1
Denmark	2.8	126	351	23
Switzerland	2.7	555	1 488	41
FRG	2.3	648	1 512	43
Sweden	2.2	28	61	14
Australia	2.0	134	267	20
GDR	1.9	319	602	17
Canada	1.8	86	152	20
Israel	1.8	18	32	7
Austria	1.7	64	107	8
Czechoslovakia	1.7	54	94	9
France	1.6	360	566	42
Japan	1.6	293	477	19
Norway	1.5	2	3	2
Totals	**3.2**	**9 888**	**31 298**	
All others	**.8**	**5 990**	**4 867**	
Grand totals	**2.3**	**15 878**	**36 165**	

Source of data on articles: 1973 *SCI*®

Source of data on citations: 1973–78 *SCI*®

accounts for 24% of all Third World articles. Also, Third World articles published in the USA came from 75 different developing countries. That is, more Third World nations found a publishing opportunity in the US than in any other publishing country.

India is a close second, publishing 3 300 Third World articles. But 98% of all Third World articles published in India were authored by Indian scientists. Eleven other developing countries managed to publish only 56 articles in Indian journals.

The other Third World publishing countries, indicated by asterisks, followed the same pattern. 75% of the Third World articles published in Argentina were authored by Argentinian scientists. For Venezuela, this figure is 97%; for Chile, 98%; for Mexico, 95%; and Brazil, 99%. Only Costa Rica published more articles from Third World countries other than itself. Of the 50 articles published in Costa Rican journals, 23 were from Costa Rica.

Significantly, no Third World publishing country had an impact of 1.5 or greater for the Third World articles it published (Table 10). Costa Rica had an impact of 1.2, the highest of all 15 Third World publishers. India followed with an impact of 1.1. Third World articles had the greatest impact when they were published in the Netherlands, USA or UK.

Journals

We can now identify the journals from the developed countries that published at least 50 articles from the Third World. Nineteen journals are shown (Table 11). Included is the overall number of articles each journal published to give an idea of the proportion from the Third World.

Six of these journals were published in the UK, the USA has five, and the Netherlands three. Switzerland and Italy account for two journals each, and one journal was published in East Germany.

Twenty-one Third World journals published at least 50 articles from the developing countries (Table 12). More than half (13) are Indian journals. The remaining eight were published in Central or South America: two each in Argentina, Chile, and Mexico; and one each in Costa Rica and Venezuela. These journals published Third World articles almost exclusively. 96% of all articles in these journals, taken together, were from the Third World.

When we rank journals that published Third World articles by

Table 11. Journals that published 50 or more Third World articles, 1973 *SCI*®

Journal	Third World articles	Total number of articles	Third World impact
Journal of Inorganic and Nuclear Chemistry (USA)	138	634	3.6
Phytochemistry (UK)	121	655	3.6
Experientia (Switzerland)	104	1 114	1.9
Transactions of the Royal Society of Tropical Medicine and Hygiene (UK)	100	312	3.6
Lancet (UK)	88	2 626	6.9
Notices of the American Mathematical Society (USA)	78	2 236	0
International Journal of Electronics (UK)			
Genetics (USA)	73	1 033	0
Pure and Applied Geophysics (Italy)	68	224	6
Biochimica et Biophysica Acta (Netherlands)	66	2 311	13.0
Physical Review B—Solid State (USA)	66	1 382	4.9
Bulletin of the World Health Organization (Switzerland)	60	177	4.0
Lettere al Nuovo Cimento (Italy)	60	571	1.3
Physica Status Solidi B—Basic Research (GDR)	60	669	1.5
Physics Letters A (Netherlands)	57	996	2.0
British Medical Journal (UK)	56	1 776	7.4
Journal of Tropical Medicine and Hygiene (UK)	52	72	2.1
Tropical and Geographical Medicine (Netherlands)	52	66	1.8
Tetrahedron Letters (USA)	51	1 406	0

impact (Table 13), no Third World journal appears among the top 25. The USA accounts for two-thirds of the journals with the highest impact for Third World articles. Seven were published in the UK, and one each in Denmark and Switzerland.

For 18 of these journals, the impact of Third World articles was greater than the journal's overall impact. That is, Third World research reported in their pages added to the overall impact of these journals.

Of the 52 Third World journals covered in the 1973 *SCI*, 14 had an impact of 1.0 or greater for articles it published from developing countries (Table 14). The highest impact for any Third World journal was 2.3. Nine Indian journals are listed. Argentina accounts for three. Mexico and Costa Rica account for one journal each.

Most-cited articles

We now examine the Third World articles that were cited at least 50 times from 1973–78. Full bibliographic information is given for 23 articles, including the institutional affiliations of all authors (Table 15). First authors who were based in India, Argentina, and Thailand each account for five most-cited Third World articles. Brazilian authors contributed four articles. Uganda, Chile, Iran, and Jamaica account for one article each.

We also checked to see if these first authors listed different institutional affiliations on other articles. For example, some may have been visiting researchers at institutions in the developed countries. On the other hand, they may have been researchers from developed countries who worked in Third World institutes for a time. We found that four first authors also listed addresses at institutions in developed countries – K.H. Graefe, R. Edelman, R.D. Keynes, and V.A. Bokisch.

In addition, five of the 23 articles were co-authored with researchers from developed countries. Thus, including a scientist from a developed country on the research team may be an effective strategy for increasing the impact of Third World research.

Although we have listed the overall number of citations each article received from 1973–78, we can identify the 'nationality' of these citations. That is, we can tell how many citations were given by authors from the same developing nation of the first author, from other Third World countries, and from developed countries.

Table 12. Third World journals that published 50 or more Third World articles, 1973 *SCI*®

Journal	Third World articles	Total number of articles	Third World impact
Current Science (India)	620	629	.8
Acta Physiologica Latino Americana (Argentina)	613	647	.3
Indian Journal of Chemistry (India)	544	553	1.9
Acta Cientifica Venezolana (Venezuela)	431	473	.1
Medicina (Argentina)	359	374	.2
Indian Journal of Pure and Applied Physics (India)	335	340	1.1
Journal of the Indian Chemical Society (India)	309	312	1.2
Indian Journal of Medical Research (India)	269	272	1.5
Indian Journal of Agricultural Sciences (India)	261	266	.4
Indian Journal of Experimental Biology (India)	221	230	1.3
Revista Medica de Chile (Chile)	181	185	6
Indian Journal of Technology (India)	157	188	.4
Indian Journal of Physics and Proceedings of the Indian Association for the Cultivation of Science (India)	108	112	.7
Archivos de Biologia y Medicina Experimentales (Chile)	102	103	0
Indian Journal of Biochemistry and Biophysics (India)	98	98	2.3
Revista de Investigacion Clinica (Mexico)	73	76	.4
Indian Journal of Genetics and Plant Breeding (India)	72	75	.4
Archivos de Investigacion Medica (Mexico)	60	65	.5
Journal of Scientific and Industrial Research (India)	57	79	1.2
Proceedings of the Indian Academy of Sciences Section B (India)	57	57	.9
Turrialba (Costa Rica)	50	71	1.2

Table 13. Top 25 journals with highest impact for Third World articles, 1973 *SCI*®

Journal	Impact of Third World articles	Impact of all articles	Third World articles	Number of citations
Physiological Reviews (USA)	192.0	103.5	1	192
Reviews of Modern Physics (USA)	59.0	86.9	2	118
Proceedings of the National Academy of Sciences of the USA (USA)	57.0	46.8	2	114
Journal of Immunology (USA)	44.2	22.6	5	221
Proceedings of the Nutrition Society (UK)	39.0	3.1	1	39
Journal of Clinical Investigation (USA)	37.5	41.6	2	75
Hormones and Behavior (USA)	36.0	12.8	2	72
Journal of Lipid Research	33.7	19.2	4	135
Cell and Tissue Kinetics (UK)	28.0	14.3	1	28
Tissue Antigens (Denmark)	25.0	13.9	1	25
Astrophysical Journal Supplement Series (USA)	24.0	30.3	1	24
Infection and Immunity (USA)	23.3	13.5	10	233
Biochemistry (USA)	23.0	26.0	4	92
Proceedings of the Royal Society Series B—Biological Sciences (UK)	23.0	15.8	1	23

continued on page 121

These 23 articles received about 1 800 citations. 86% were from authors in developed countries. 10% were from the first author's developing country. Only 40% were from authors in other Third World nations. Thus, whether or not a Third World article is highly-cited depends on the recognition it gets from authors in the developed countries.

Part of the reason for this is that no Third World journal published the most-cited articles from the developing countries. Thirteen of the 23 most-cited articles appeared in US journals. Seven were published in the UK. Two were published in the Netherlands, and one in West Germany.

We have not yet discussed, in a cognitive sense, the most active areas of Third World research. We have shown some research their scientists do best by examining the most-cited articles from the Third World. But any conclusion drawn from such a limited sample of Third World articles would be tentative, at best.

Clustering method

At *ISI*, a method has been developed to bring into view the cognitive structure of science.[15] Here, I limit myself to a brief explanation of what is actually a complex procedure (Figure 2).

When we record the references in an article, we also keep track of the *pairs* of papers it cited together, or co-cited. When the same pairs of papers are cited together with other papers by many authors, a cluster of research begins to form. The idea underlying this method is that co-cited articles share a common topic, subject area, or method. That is, when an author cites two papers together, he or she indicates that both papers applied to some aspect of the research he or she performed. When the same cluster of papers are co-cited by several authors, a cognitive link is established between the research these authors perform. That is, the citing authors reveal what research area they 'belong to' through their citations. By examining the titles of the citing articles, we get an idea of the cognitive content of their research concentration. That is, the citing authors themselves provide the words to define what their research area is about.

At another level, we can also identify co-citation links between clusters. This is possible when authors cite together articles included in different clusters. Again, we assume

Table 13 continued

Chemical Society Reviews (UK)	21.5	34.9	2	43
Human Pathology (USA)	20.5	8.0	2	41
Nephron (Switzerland	20.5	6.5	2	41
Journal of Membrane Biology (USA)	20.0	14.8	7	140
Inorganic Chemistry (USA)	19.4	11.9	9	175
Philosophical Transactions of the Royal Society of London Series A–Mathematical and Physical Sciences (UK)	19.0	7.9	2	38
Blood (USA)	18.3	12.0	3	55
Annual Review of Microbiology (USA)	17.5	30.9	2	35
Endocrinology (USA)	17.4	21.4	7	122
Clinical and Experimental Immunology (UK)	16.8	23.0	5	84
Journal of Physiology London (UK)	16.4	11.0	16	262

Table 14. Third World journals with an impact of 1.0 or greater for Third World articles, 1973 *SCI*®

Journal	Impact of Third World articles	Impact of all articles	Third World articles	Citations
Indian Journal of Biochemistry and Biophysics (India)	2.3	2.3	98	223
Indian Journal of Chemistry (India)	1.9	1.9	544	1 027
Anales de la Asociacion Quimica Argentina (Argentina)	1.8	1.8	39	71
Acta Endocrinologica Panamericana (Argentina)	1.6	1.2	7	11
Indian Journal of Medical Research (India)	1.5	1.5	269	404
Phyton (Argentina)	1.5	1.1	8	12
Indian Journal of Experimental Biology (India)	1.3	1.2	221	286
Journal of the Indian Chemical Society (India)	1.2	1.2	309	381
Journal of Scientific and Industrial Research (India)	1.2	1.3	57	71
Phytomorphology (India)	1.2	1.6	4	5
Turrialba (Costa Rica)	1.2	1.0	50	58
Indian Journal of Pure and Applied Physics (India)	1.1	1.1	335	384
Patologia (Mexico)	1.1	1.3	16	18
Nucleus (India)	1.0	1.3	26	25

that authors who use the same clusters of articles are engaged in similar types of research. The more frequently two clusters of articles are co-cited, the clusters, we use this relationship to determine how close or far apart clusters should be depicted in relation to each other.

We used the 1981 *SCI* file to see what clusters were cited in articles from Third World countries. This gives an understanding of the *current* areas of activity in Third World science. We started by identifying all articles from Third World countries in the 1981 SCI. We then identified all the articles they cited that year. We then determined which 1981 articles from developed countries referred to the same group of papers cited by Third World authors. These data were entered into our computer, and more than 2 000 clusters of co-cited papers were generated.

Before we identified clusters that were cited by Third World articles, we decided to separate India from the developing countries. As we have seen, India overshadows the rest of the Third World in the number of scientific articles it authors and publishes. By considering India separately, we have a clearer picture of Third World scientific activity.

Maps of clusters

Thirty-nine clusters were cited by at least 15 articles from the Third World, excluding India (see Figure 3). Each circle represents a single cluster. The shading indicates different disciplines of research. Connecting lines indicate co-citation links between these disciplines.

The clusters are densely packed and highly interconnected for a good reason. 90% of them deal with topics in closely related fields – clinical and biomedical science. Thirteen clusters concentrate on various aspects of immunity, particularly to viral and bacterial diseases; seven deal with hormones and fertility; and four discuss circulatory and heart disease. The ten clusters on 'miscellaneous' topics in clinical and biomedicine concern cancer, neurotransmission, physiology, metabolism, and other topics. The remaining five clusters in this figure are in fields other than clinical and biomedicine – two in physics, one in astronomy, and two in agricultural and environmental sciences.

Obviously, Third World research activity is focused on clinical and biomedical science. That is, developing country scientists tend to cite clusters of articles in clinical and biomedical research almost exclusively. But what specific areas of clinical and biomedical research are cited *most* frequently by Third World scientists? This can be determined by considering the proportion of all articles citing these clusters that were from the Third World.

Third World articles amounted to at least 15% of all citing articles for 11 clusters. As we can see in Figure 4, the level of Third World participation is significant in seven clusters of research on immunology and infectious disease.

Another area of Third World scientific expertise is in hormone and fertility research. Third World scientists also accounted for a significant proportion of all papers citing research in the agricultural cluster.

The cluster names read like an agenda of Third World concerns: diseases transmitted by parasites, bacteria, and viruses; immune responses to these and other infectious diseases; hormones, steroids, and fertility; and grains and legumes. Except for the last, all Third World science is focused on clinical and biomedical research. Of course, Third World scientists do

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Table 15. Third World articles cited at least 50 times from 1973–78

Citations 1973–1978 *SCI*®				
Total	First author's country	Third World	Developed world	Bibliographic data
192	8	1	183	Paintal A.S. Vagal sensory receptors and their effects. *Physiol Rev* 53:159–227, 1973. Delhi Univ, Vallabhbhai Patel Chest Inst, Delhi, India.
166	13	1	152	Mendes N F, Tolnai M E A, Silveira N P A, Gilbertsen R B and Metzgar R S. Technical aspects of the rosette tests used to detect human complement receptor (B) and sheep erythrocyte-binding (T) lymphocytes. *J Immunol* 111:860–7, 1973. Escola Paulista Med, Dept Microbiol, Immunol Parasitol, Sao Paulo, Brazil; Duke Univ Med Ctr, Dept Microbiol Immunol, Durham, NC.
118	1	0	117	Punsalang A P and Sawyer W D. Role of pili in the virulence of *Neisseria gonorrhoeae*. *Infec Immunity* 8:255–63, 1973. Mahidol Univ, Dept Microbiol; Rockefeller Found, Bangkok, Thailand.
109	16	0	93	Behrens N H, Carminatti H, Staneloni R J, Leloir L F and Cantarella A I. Formation of lipid-bound oligosaccharides containing mannose. Their role in glycoprotein synthesis. *Proc Nat Acad Sci US* 70:3390–4, 1973. Univ Buenos Aires, Inst Invest Bioquim, "Fundación Campomar", Buenos Aires, Argentina.
88	10	16	62	Sirisinha S, Suskind R, Edelman R, Charupatana C and Olson R E. Complement and C3-proactivator levels in children with protein-calorie malnutrition and effect of dietary treatment. *Lancet* 1:1016–20, 1973. Mahidol Univ, Dept Microbiol; SEATO Med Res Lab, Dept Virol, Bangkok, Thailand; Chiang Mai Univ, Anemia Malnutr Res Ctr, Chiang Mai, Thailand; St. Louis Univ Sch Med, Dept Biochem Pediat, St. Louis, MO.
68	10	11	47	Edelman R, Suskind R, Olson R E and Sirisinha S. Mechanisms of defective delayed cutaneous hypersensitivity in children with protein-calorie malnutrition. *Lancet* 1:506–8, 1973. SEATO Med Res Lab, Dept Virol, Bangkok, Thailand; Chiang Mai Univ, Anemia Malnutr Res Ctr, Chiang Mai, Thailand; St. Louis Univ Sch Med, Dept Biochem Pediat, St. Louis, MO; Mahidol Univ, Dept Microbiol, Bangkok, Thailand.
65	1	0	64	Thongthai C and Sawyer W D. Studies on the virulence of *Neisseria gonorrhoeae*. I. Relation of colonial morphology and resistance to phagocytosis by polymorphonuclear leukocytes. *Infec. Immunity* 7:373–9, 1973. Mahidol Univ, Dept Microbiol; Rockefeller Found, Bangkok, Thailand.
61	5	6	50	Bokisch V A, Top F H, Russell P K, Dixon F J and Müller-Eberhard H J. The potential pathogenic role of complement in dengue hemorrhagic shock syndrome. *N. Engl. J. Med.* 289:996–1000, 1073. SEATO Med Res Lab, Dept Virol, Bangkok, Thailand; Scripps Clin Res Found, Dept Exp Pathol, La Jolla, CA; Walter Reed Army Inst Res, Div Communic Dis Immunol, Washington, DC.
61	14	0	47	Leloir L F, Staneloni R J, Carminatti H and Behrens N H. The biosynthesis of a N,N′-diacetylchitobiose containing lipid by liver microsomes. A probable dolichol pyrophosphate derivative. *Biochem Biophys Commun* 52:1285–92, 1973. Univ Buenos Aires, Inst Invest Bioquim, "Fundación Campomar", Buenos Aires, Argentina.

368

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Table 15. continued

80	2	2	76	Jacob G and Maris Th A J. Quasi-free scattering and nuclear structure. II. *Rev Mod Phys* 45:6–21, 1973. Univ Fed Rio Grande Sul, Inst Fis, Porto Alegre, Brazil.
80	2	9	69	Karim S M M, Carter D C, Bhana D, Ganesan P A. Effect of orally administered prostaglandin E_2 and its 15-methyl analogues on gastric secretion. *Brit Med J.* 1:143–6, 1973. Makerere Univ Med Sch, Kampala, Uganda.
79	4	2	73	Garay R P and Garrahan P J. The interaction of sodium and potassium with the sodium pump in red cells. *J Physiol London* 231:297–325, 1973. Univ Buenos Aires, Dept Quim Biol, Buenos Aires, Argentina.
79	23	0	56	Graefe K H, Stefano F J E and Langer S Z. Preferential metabolism of (−)-^{3}H-norepinephrine through the deaminated glycol in the rat vas deferens. *Biochem Pharmacol* 22:1147–60, 1973. Consejo Nac Invest Cientif Tecn Inst, Invest Farmacol, Buenos Aires, Argentina.
72	10	0	62	Ramachandran G N, Lakshminarayanan A V and Kolaskar A S. Theory of the non-planar peptide unit. *Biochim Biophys Acta* 303:8–13, 1973. Indian Inst Sci, Mol Biophys Unit, Bangalore, India; Univ Chicago, Dept Biophys, Chicago, Ill.
70	3	0	67	Raheja R K, Kaur C, Singh A and Bhatia I S. New colorimetric method for the quantitative estimation of phospholipids without acid digestion. *J Lipid Res* 14:695–7, 1973. Punjab Agricult Univ, Dept Chem Biochem, Ludhiana, India.
68	3	4	61	Keynes R D, Rojas E, Taylor R E and Vergara J. Calcium and potassium systems of a giant barnacle muscle fibre under membrane potential control. *J Physiol London* 229:409–55, 1973. Univ Chile, Lab Cell Physiol, Casilla, Chile.
58	8	3	47	Reinhold J G, Nasr K, Lahimgarzadeh A and Hedayati H. Effects of purified phytate and phytate-rich bread upon metabolism of zinc, calcium, phosphorus, and nitrogen in man. *Lancet* 1:283–8, 1973. Pahlavi Univ Sch Med, Inst Nucl Med, Shiraz, Iran.
58	5	1	52	Seakins M, Gibbs W N, Milner P F and Bertles J F. Erythrocyte Hb-S concentration. An important factor in the low oxygen affinity of blood in sickle cell anemia. *J Clin Invest* 52:422–32, 1973. Univ West Indies, Dept Chem Hematol, Kingston, Jamaica; St. Luke's Hosp Ctr, Hematol Div, New York, NY.
57	4	7	46	Chandra R K. Reduced bactericidal capacity of polymorphs in iron deficiency. *Arch Dis Child.* 48:864–6, 1973. All India Inst Med Sci, Dept Paediat, New Delhi, India
54	10	0	44	Karniol I G and Carlini E A. Pharmacological interaction between cannabidiol and Δ^9-tetrahydrocannabinol. *Psychopharmacol* 33:53–70, 1973. Escola Paulista Med, Dept Psicobiol, Sao Paulo, Brazil.
53	7	3	43	Blaquier J A and Calandra R S. Intranuclear receptor for androgens in rat epididymis. *Endocrinology* 93:51–60, 1973. Inst Biol Med Exp, Lab Esteroides, Buenos Aires, Argentina.
53	2	0	51	Srivastava R K, Kulshrestha V K, Singh N and Bhargava K P. Central cardiovascular effects of intracerebroventricular propranolol. *Eur J Pharmacol* 21:222–9, 1973. Univ Lucknow, King George's Med Coll, Lucknow, India.
52	14	7	31	Toma H E and Malin J M. Properties and reactivity of some pentacyanoferrate (II) complexes of aromatic nitrogen heterocycles. *Inorg Chem* 12:1039–45, 1973. Univ Sao Paulo, Inst Quim, Sao Paulo, Brazil.

Citation 1973–1978 *SCI*®

research in physics, chemistry, mathematics, and other fields. But they are *most* active in clinical and biomedical research.

Indian clusters

When we examine Indian science and exclude the rest of the Third World (see Figure 5), we see that 32 clusters were cited by at least 16 articles from India. The smaller structure that stands apart to the left includes seven clusters. They are all in chemistry: these clusters discuss valence states, electron density, and phase transitions of various crystals and liquids.

The larger structure to the right includes 25 clusters. They can be organized into three separate fields of concentration. Eleven clusters deal with the synthesis and properties of various metal and ligand complexes. For convenience, we categorize them as being in chemical physics.

Nine clusters concentrate on biochemistry, and deal with protein analysis, plant genetics, DNA conformation, and the structure and bioactivity of various substances. One of these discusses viral gastroenteritis, the only biomedical cluster on the map of Indian science. But only 3% of the articles citing this cluster were from India.

The remaining five clusters, deal with physics. They discuss quantum chromodynamics, quark models, unified field theories, and other topics.

Unlike the other developing countries, Indian science is not focused on any one field. It is interesting that Indian science was not active in clinical and biomedical research, which preoccupied the rest of Third World science. Instead, Indian scientists cited research in biochemistry, physics, chemistry, and chemical physics. But Indian research activity was concentrated in chemical physics.

Indian articles amounted to at least 25% of all citing articles for 12 clusters (see Figure 6). Nine of these clusters deal with metal and ligand complexes, which we have categorized as topics in chemical physics. In the cluster named 'Stability Constants of Metal Complexes with Asymmetric Multidentate Ligands', the level of Indian participation was very high. More than 100 articles published in 1981 cited this cluster, and 77% were from India.

Chemistry is another active area of Indian science. These clusters deal with chemical properties of alcohol and various crystals. And one cluster

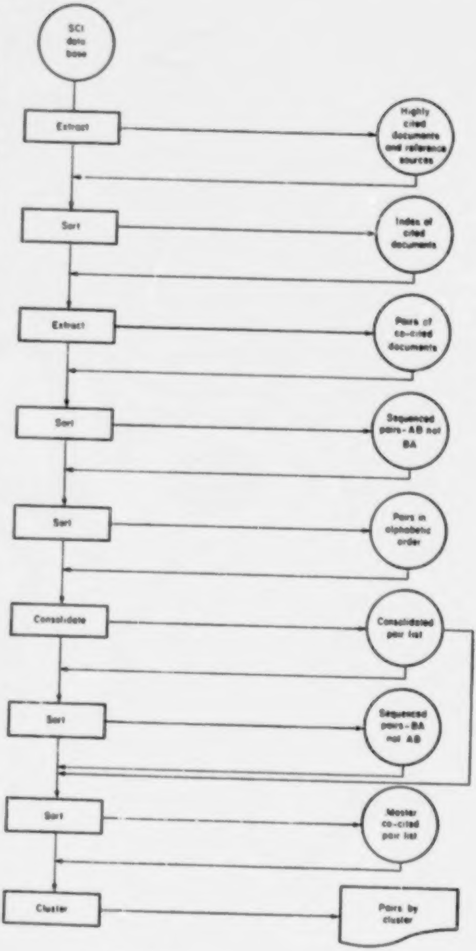
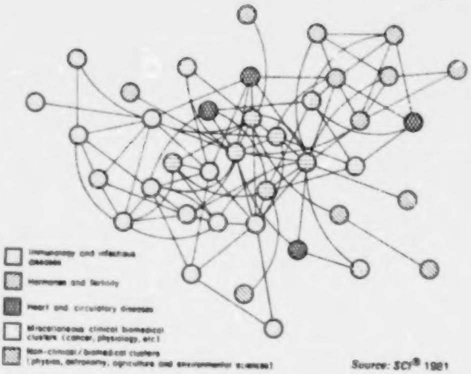

SCI data base
Extract
Highly cited documents and reference sources
Sort
Index of cited documents
Extract
Pairs of co-cited documents
Sort
Sequenced pairs - AB not BA
Sort
Pairs in alphabetic order
Consolidate
Consolidated pair list
Sort
Sequenced pairs - BA not AB
Sort
Master co-cited pair list
Cluster
Pairs by cluster

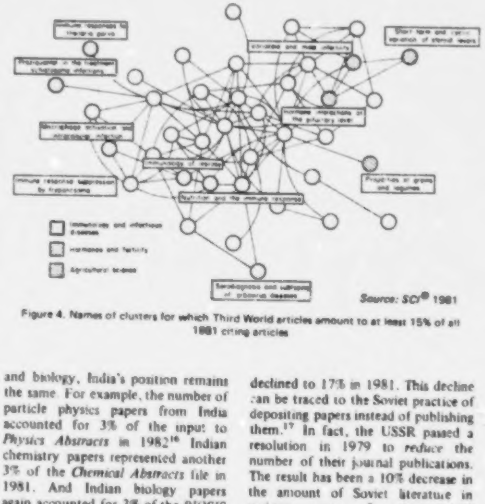
Figure 2. Diagrams of ISI's clustering method

Source: SCI® 1981


Immunology and infectious diseases
Hormones and fertility
Heart and circulatory diseases
Miscellaneous clinical biomedical clusters (cancer, physiology, etc)
Non-clinical / biomedical clusters (physics, astronomy, agriculture and environmental sciences)

Source: SCI® 1981

Figure 3. Clusters of research cited by 1981 articles from Third World countries, excluding India


Praziquantel in the treatment of schistosoma infections
Chlamydiae and male infertility
Short term and cyclic variation of steroid levels
Hormone interactions at the pituitary level
Macrophage activation and intracellular infection
Immunobiology of leprosy
Immune response suppression by trypanosomes
Properties of grains and legumes
Nutrition and the immune response
Immunology and infectious diseases
Hormones and fertility
Agricultural science

Source: SCI® 1981

Figure 4. Names of clusters for which Third World articles amount to at least 15% of all 1981 citing articles

in biochemistry, colored blue, showed a significant level of participation by Indian researchers. Of the 108 articles that cited this cluster on 'Diallyl Analysis and other Biometrical Genetics Studies', 27 were from India.

Conclusions

When we talk about Third World science, we have to distinguish between India and the rest of the developing world. In many respects, Indian science is unlike the mainstream of Third World research.

First, Indian science is not clustered in clinical or biomedical research. Of course Indian scientists do research in clinical and biomedicine, but they are not the *most* active areas of Indian research. In contrast, the rest of the Third World actively participates in clinical and biomedical research.

Also, Indian science is clustered in research fields that other Third World scientists have not significantly penetrated – biochemistry, physics, chemistry, and chemical physics. And the level of participation of Indian scientists in chemical physics is especially remarkable.

India also stands apart from all other developing nations in the number of articles its scientists authored. Remember that half the 16000 Third World articles in the 1973 *SCI* were authored in India. And India's dominant position in the Third World has remained constant over the years. For example, there were 22000 Third World articles in the 1978 *SCI* database. Of these, 52% were authored in India. The 1981 *SCI* database included 27000 Third World articles, and 49% of these were from India.

India overshadows the rest of the Third World in the number of articles published in its scientific press, as well. Indian journals accounted for 60% of all 1973 articles published in the Third World.

Thus, we get a skewed impression of Third World science when we do not take separate account of India. For example, 5% of the 353000 articles in the 1973 *SCI* were authored in the Third World. But when India's contribution is subtracted the proportion of Third World articles drops to only 2%. And 2% of the 1973 *SCI* articles were published in Third World journals. But this drops to less than 1% when India is excluded.

When we consult data from other indexing services that claim to be comprehensive for the world's publications in physics, chemistry, and biology, India's position remains the same. For example, the number of particle physics papers from India accounted for 3% of the input to *Physics Abstracts* in 1982[16] Indian chemistry papers represented another 3% of the *Chemical Abstracts* file in 1981. And Indian biology papers again accounted for 3% of the *BIOSIS* file from 1978 to 1981.

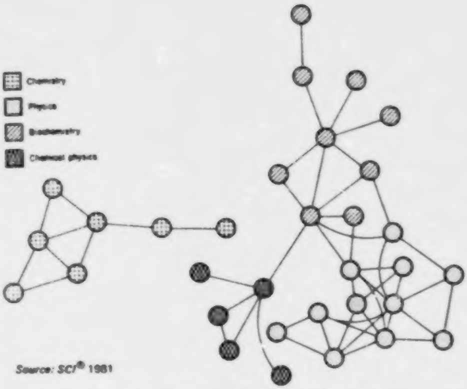
It is interesting to note the USSR's position in this context. In 1970, Soviet chemistry papers represented 24% percent of the *Chemical Abstracts* file. This number declined to 17% in 1981. This decline can be traced to the Soviet practice of depositing papers instead of publishing them.[17] In fact, the USSR passed a resolution in 1979 to *reduce* the number of their journal publications. The result has been a 10% decrease in the amount of Soviet literature in various areas of science and technology.[17]

I can not say whether the data presented indicate that Third World research, with or without India, is 'under' -represented in international scientific journals. Granted, articles authored and published in Third World countries amount to a very small proportion of the international scientific press. But this might be explained in several ways.

For example, Third World countries cannot afford to devote a large percentage of their gross national products to scientific research. This has an obvious effect on the availability of scientific instruments, supplies, and materials in developing countries. Without all this, the conduct of research in the Third World is diminished.

Or, Third World countries may not have a large enough research community to generate significant numbers of articles. This would also affect the number of scientific journals the research community can support.

I am sure there are many other factors that might explain why Third World scientific output amounts to such a small proportion of the international scientific output. Until these factors are analysed, we cannot decide if the coverage of Third World research is equitable. And until we make that decision, we cannot talk about the need for restructuring the world scientific press. This lecture is intended to serve as a point of departure for what I hope will be a continuing analysis of the level and impact of Third World science. It remains to be seen whether or not a New Scientific Information Order is needed.



Source: SCI® 1981

Figure 5. Clusters of research cited by 1981 articles from India, excluding the rest of the Third World

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(reprinted from *Current Contents* (15), 11 April 1977, pp 5–11, translation of Garfield, E. "La science française est-elle trop provinciale?", *Recherche*, 7, 1976, pp 757–760

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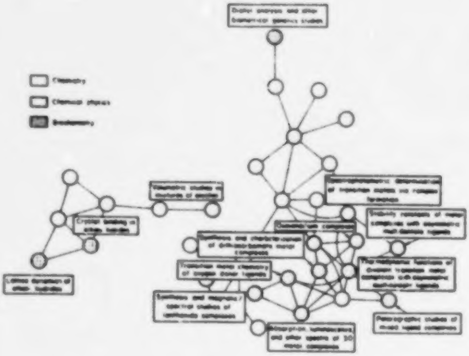

Figure 6. Names of clusters for which Indian articles amount to at least 25% of all citing articles

Source: SCI® 1981

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Nobelists in the 300 Most-Cited Authors, 1961-76

		TOTAL CITATIONS 1961 - 1976
ANDERSON, PW	NOBEL/PHYSICS/77	3,838
ANFINSEN, CB	NOBEL/C/72	4,942
AXELROD, J	NOBEL/M or P/70	15,769
BALTIMORE, D	NOBEL/M or P/75	5,270
★BENACERRAF , B	NOBEL/M or P/80	9,197
★BERG, P	NOBEL/C/80	5,307
BLUMBERG, BS	NOBEL/M or P/76	6,029
★BROWN, HC	NOBEL/C/79	10,288
DEDUVE, CR	NOBEL/M or P/74	4,178
ECCLES, JC	NOBEL/M or P/63	4,579
EDELMAN, GM	NOBEL/M or P/72	6,797
FLORY, PJ	NOBEL/C/74	5,538
GELL-MANN, M	NOBEL/PHYSICS/69	4,912
GUILLEMIN, R	NOBEL/M or P/77	4,200
★HOFFMAN, R	NOBEL/C/81	7,969
★HUBEL, DH	NOBEL/M or P/81	4,474
JACOB, F	NOBEL/M or P/65	10,383
KHORANA, HG	NOBEL/M or P/68	6,620
KORNBERG, A	NOBEL/M or P/59	6,706
KREBS, HA	NOBEL/M or P/53	5,146
LIPMANN, F	NOBEL/M or P/53	5,019
LIPSCOMB, WN	NOBEL/C/76	6,364
MONOD, J	NOBEL/M or P/65	6,945
MOORE, S	NOBEL/C/72	5,619
OCHOA, S	NOBEL/M or P/59	4,172
PERUTZ, MF	NOBEL/C/62	4,734
PALADE, GE	NOBEL/M or P/74	11,242
★SAMUELSSON B	NOBEL/M or P/82	5,849
SCHALLY, AV	NOBEL/M or P/77	10,386
SUTHERLAND, EW	NOBEL/M or P/72	11,644
★VANE J R	NOBEL/M or P/82	6,292
★WEINBERG, S	NOBEL/PHYSICS/79	7,349
★WIESEL, TN	NOBEL/M or P/81	4,605
WOODWARD, RB	NOBEL/C/65	4,044
YALOW, RS	NOBEL/M or P/77	5,595

★ Awarded Nobel Prize after list published.

Nobel Prizewinners on 1000 most-cited authors list

NAME	YEAR	PRIZE
Anderson, Philip Warren	1977	Physics
Anfinsen, Christian Boehmer	1972	Chemistry
Axelrod, Julius	1970	Medicine
Baltimore, David	1975	Medicine
Barton, Derek Harold Richard	1969	Chemistry
Benacerraf, Baruj	1980	Medicine
Berg, Paul	1980	Chemistry
Bloembergen, Nicolaas	1981	Physics
Blumberg, Baruch Samuel	1976	Medicine
Brown, Herbert C.	1979	Chemistry
Dausset, Jean	1980	Medicine
de Duve, Christian Rene	1974	Medicine
Edelman, Gerald Maurice	1972	Medicine
Fischer, Ernst Otto	1973	Chemistry
Flory, Paul John	1974	Chemistry
Glashow, Sheldon Lee	1979	Physics
Guillemin, Roger	1977	Medicine
Hoffmann, Roald	1981	Chemistry
Hubel, David H.	1981	Medicine
Jacob, Francois	1965	Medicine
Katz, Bernard	1970	Medicine
Khorana, Har Gobind	1968	Medicine
Klug, Aaron	1982	Chemistry
Kornberg, Arthur	1959	Medicine
Krebs, Hans Adolf	1953	Medicine
Lee, Tsung-Dao	1957	Physics
Lipmann, Fritz A.	1953	Medicine
Lipscomb, William Nunn	1976	Chemistry
Mitchell, Peter (Dennis)	1978	Chemistry
Monod, Jacques (Lucien)	1965	Medicine
Mott, Nevill Francis	1977	Physics
Nathans, Daniel	1978	Medicine
Nirenberg, Marshall Warren	1968	Medicine
Ochoa, Severa	1959	Medicine
Palade, George E.	1974	Medicine
Perutz, Max Ferdinand	1962	Chemistry
Richter, Burton	1976	Physics
Samuelsson, Bengt	1982	Medicine
Sanger, Frederick	1958/1980	Chemistry
Schally, Andrew Victor	1977	Medicine
Sutherland, Earl Wilbur	1971	Medicine
Temin, Howard Martin	1975	Medicine
Vane, John R.	1982	Medicine
Weinberg, Steven	1979	Physics
Wiesel, Torsten N.	1981	Medicine
Wilkinson, Geoffrey	1973	Chemistry
Wilson, Kenneth G.	1982	Physics
Woodward, Robert Burns	1965	Chemistry
Yalow, Rosalyn (Sussman)	1977	Medicine

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Current Comments®

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Citation Classics—Four Years of the Human Side of Science

Number 22 June 1, 1981

Most readers of *Current Contents*® (*CC*®) are by now familiar with the *Citation Classics* feature which began in 1977. It seems to me that after four years of this weekly feature an evaluation is now in order.

In reviewing *Citation Classics*, one is immediately struck by the fact that our goals have changed somewhat in the past four years. Originally, we planned to draw our classics from a "group of 500 papers most-cited during the years 1961-1975."[1] By 1979, however, we realized the consequences of this narrow approach to selecting papers. Most of the 500 most-cited papers came from the life sciences. There are many reasons for this. Consequently, in 1979, in order to make our coverage more comprehensive and representative of all the fields of science, we began to publish six different classics each week—one for each edition of *CC*,[2] except *Arts & Humanities*. And in early 1981, to eliminate our backlog we began publishing two classics in *CC/Life Sciences* per week.

While citation frequency is a prime indication of a paper's impact, we have not rigidly adhered to any particular level of citation. A paper in one of the basic engineering sciences may be a classic even if it has been cited only 30 or 40 times. This would still be orders of magnitude greater than the number of citations received by millions of average papers even in the life sciences. And in the social sciences, as in engineering, books often are as important as journal articles so we must apply separate criteria for them.

Apart from expanding the definition of a *Citation Classic* so that it comes closer to what scientists perceive as a classic, our basic goal in publishing *Citation Classics* has remained the same: to present the human side of science while paying tribute to diverse advances in science and scholarship. These commentaries were designed to allow scientists to talk about their major works from a personal standpoint, revealing what prompted the research, the contributions of coauthors, and obstacles that were encountered in both research and publication—in short, those details that are rarely revealed in formal scientific publication. The more than 750 classics published so far have exceeded my expectations.

I think it is unfortunate that scientific journals do not have the flexibility or the sense to provide some of this background. But in the context of a scientific journal, it may not be particularly relevant to learn, for example, that Martin Seligman's paper on the laws of learning resulted from an illness he contracted after eating béarnaise sauce.[3] In this particular instance, although the illness was not related to the eating experience, it so conditioned Seligman that he

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still (15 years later) cannot eat béarnaise sauce. The paper grew out of his conjectures on why this is so.

Once we have determined through various citation analyses that a paper is a milestone paper in its field, how do we get the essays, or vignettes, written? First, we contact the author and ask him or her to prepare a commentary. The author also receives an author's guide which explains the feature and the kind of information we'd like included in the commentary. The author also receives samples of published *Citation Classics*. We encourage the author to stress the personal factors involved in getting the original paper published and we ask for information about coauthors, and how and where the work took place. One of the most important questions is why he or she thinks the publication has been cited so often. In order to make the essay topical for readers interested in following up on the subject, we ask the author to cite a more recent review article or publication. We also ask each author to mention any awards or honors that resulted from the research. This is further confirmation that it is indeed a classic. I take particular pleasure in noting that the authors of several classics went on to be recipients of the National Academy of Sciences award for scientific reviewing![4,5]

Since we began *Citation Classics* in 1977, we've extended invitations to over 2,000 authors. It is somewhat disappointing to me that only about one third have accepted. Of course if we had a better response we'd have written fewer letters, but eventually we would have covered all of them. Another 100 authors have promised to send manuscripts, but have yet to do so. We telephone most to reinvite them. Nearly half of those asked, however, have simply never acknowledged our letters. We can't even be sure they were delivered. This is to be expected since many have moved.

I had been concerned about the possibility that an inordinate percentage of refusals and nonresponses involved foreign authors. What if some of these authors are unable or unwilling to prepare a commentary in English? But in fact about 30 percent of nonrespondents are from outside the US, closely comparable to what one would expect from the number of classics published. It also turns out that the refusal rate of about 50 percent for authors in English-speaking countries is almost identical for the other countries. This would dispel the notion of an English-language bias.

The average age of the classics covered today is 19 years, and the vast majority of classics (454) were published in the 1960s. One hundred sixty-one were published in the 1950s, 73 in the 1970s, 35 in the 1940s, and five in the 1930s. Of course, some authors have died since writing their classic articles, but we do accept surrogates if a coauthor is not available. About 100 authors responded but refused our invitation.

I am delighted to report that 14 Nobel prizewinners, not to mention dozens of other prizewinners and academy members, have taken the time to write about their *Citation Classics*. These positive responses make the varied reasons for refusal hard to comprehend. Some authors claimed a lack of time, others a lack of interest in their original papers, and still others expressed a feeling that it just wasn't worth the effort, especially if they have left the field. I think it is unfortunate because these authors deny their colleagues and society a perspective on their work that they alone can provide. In the future, we intend to ask one of their colleagues or students if they will prepare essays. These commentaries may become a part of our

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forthcoming *Encyclopedic Atlas of Science*. And we hope to publish collections of these essays for use by graduate students and others interested in the way science actually works.

From the time the first *Citation Classics* was published on January 3, 1977, until the last classic of 1980 on December 29, 1980, we covered 728 classics in *CC*. By the end of 1981 this figure will exceed 1,000! Figure 1 provides a breakdown for the 24 countries represented at the time the authors published their classic papers. Most authors came from English-speaking countries. The US accounted for 502 papers, over two thirds; the United Kingdom, 93; Canada, 29; and Australia, 19. All but three of the original publications were in English. Of the remaining three, two were published in French, and one in German. One author claimed that when he first published his results in French journals the work received little attention.[6] While this was not necessarily true for papers published in the 1950s, it is clear that publication in any foreign language today can delay recognition of significant work.[7] Not all scholars accept this explanation alone, however. Derek J. de Solla Price, Yale University, for example, suggests that the preponderance of papers from English-speaking countries may be due in part to what he feels is an English-speaking bias in our citation index.[8] I would argue that our bias is towards the high impact journals, regardless of their language. And it is quite possible that a few Russian authors are overlooked because citations to their papers may be fragmented in vernacular and translation journals.

Figure 1: Countries which produced original *Citation Classics* articles, with the number of papers from each.

Country		Number of Papers
US		502
United Kingdom		93
England	84	
Scotland	8	
Wales	1	
Canada		29
Australia		19
Sweden		11
Federal Republic of Germany (FRG)		6
France		5
Denmark		3
Israel		3
Japan		2
South Africa		2
Switzerland		2
Argentina		1
Belgium		1
Finland		1
German Democratic Republic (GDR)		1
India		1
Mexico		1
The Netherlands		1
New Zealand		1
Spain		1
USSR		1
Not available		41

Nearly 250 different institutions are represented in Figure 2 which lists addresses for the original publications. However, since many authors have moved, they now work at over 315 different institutions which are listed in Figure 3. If overlaps are eliminated, there are over 425 institutions. To save space we have not repeated the names of institutions named in Figure 2. The fact that so many new institutions show up in Figure 3 illustrates the growth of research worldwide and the migration of classic authors to other institutions. While most worked at academic institutions, industry and government are well-represented. These figures are based on the use of the addresses for first authors only. I doubt that including second authors would change much in this case.

As could be expected from the examples set by our other citation studies, a few institutions dominate the lists. The combined campuses of the University of California, for example, top both lists. Forty classics were written at California, but 55 authors are now currently at work there. The migration to the west-

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Figure 2: The institutional affiliations of *Citation Classics* authors, at the time they wrote their classic papers, with the number of authors from each.

University of California		40
Berkeley	17	
Davis	3	
Irvine	1	
Livermore	1	
Los Angeles	7	
Riverside	2	
San Diego	3	
San Francisco	5	
Santa Barbara	1	
Natl. Inst. Health		25
Natl. Cancer Inst.	10	
Natl. Heart, Lung and Blood Inst.	6	
Natl. Inst. Allergy and Infectious Disease	3	
Natl. Inst. Arthritis, Metabolism and Digestive Disorders	2	
Natl. Inst. Child Health and Human Development	1	
Natl. Inst. Dental Res.	3	
University of Wisconsin		24
Harvard University		22
Bell Labs., Murray Hill and Holmdel, NJ		18
University of London		18
Birbeck Coll.	1	
Imperial Coll. Science and Technology	3	
Inst. Cancer Res.	2	
Inst. Child Health	1	
Inst. Psychiatry	1	
Lister Inst. Preventive Med.	1	
Middlesex Hosp. Med. Sch.	4	
Royal Holloway Coll.	1	
Royal Postgraduate Med. Sch.	3	
University Coll.	1	
Johns Hopkins Univ. and Hosp.		14
Stanford University		13
California Inst. Technology		11
Columbia University		11
University of Cambridge		11
University of Chicago		11
University of Illinois		11
University of Pennsylvania		10
University of Washington, Seattle, WA		10
Cornell University		9
University of Texas		9
General Electric Co., Schenectady, NY		7
Massachusetts Inst. Technology		7
University of Minnesota		7
Yale University		7
Carnegie-Mellon University		6
Oxford University		6
US Department of Agriculture		6
Agriculture Res. Ctr., Beltsville, MD	3	
Cereal Science and Foods Lab., Peoria, IL	1	
Agricultural Marketing Serv., Beltsville, MD	1	
Regional Poultry Res. Lab., East Lansing, MI	1	
University of Michigan		6
University of Rochester		6

Brookhaven Natl. Labs., Upton, NY	5
Duke University	5
McGill University, Montreal, Canada	5
Natl. Bureau of Standards, Washington, DC	5
Natl. Res. Council of Canada	5
State University of New York	5
University of Colorado	5
Washington University, St. Louis, MO	5
Commonwealth Scientific and Industrial Res. Organization (CSIRO), Australia	4
New York University	4
Northwestern University	4
Pennsylvania State University	4
University of Edinburgh	4
University of Lund, Sweden	4
University of Melbourne	4
University of Oregon	4
University of Toronto	4
IBM Corp., Yorktown Heights, NY	3
Indiana University	3
Iowa State University	3
Mayo Clinic and Foundation, Rochester, MN	3
Michigan State University	3
NASA, Greenbelt, MD	3
Purdue University	3
RCA Labs., New York, NY	3
Rockefeller University	3
US Naval Res. Lab., Washington, DC	3
University of Adelaide	3
University of Pittsburgh	3
University of Reading, UK	3
Weizmann Inst. Science, Rehovot, Israel	3
Albert Einstein Coll. Med., New York, NY	2
Avco-Everett Res. Lab., Everett, MA	2
Baylor University, Waco, TX	2
Brown University, Providence, RI	2
Bucknell University, Lewisburg, PA	2
Canadian Department of Agriculture	2
Case Western Reserve University	2
Colonial Sugar Refining Co., Indooroopilly, Australia	2
Dow Chemical Co., Midland, MI	2
E. I. Du Pont de Nemours and Co., Wilmington, DE	2
Haskins Labs., New Haven, CT	2
Humble Oil Co., Houston, TX	2
Inst. Pasteur, Paris, France	2
Massachusetts General Hosp., Boston, MA	2
Medical Res. Council, UK	2
Mount Sinai Sch. Med., New York, NY	2
Natl. Inst. Med. Res., Mill Hill, UK	2
Natl. Physical Lab., Middlesex, UK	2
Ohio State University	2
Oregon State University	2
Peter Bent Brigham Hosp., Boston, MA	2
Princeton University	2
Public Health Res. Inst. of the City of New York	2
Rothamsted Experimental Station, Harpenden, UK	2

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State University of Iowa	2
UK Atomic Res. Estab., Harwell, UK	2
US Air Force	2
US Army	2
US Public Health Serv.	2
US Steel Corp., Pittsburgh, PA	2
University of Birmingham	2
University of Bristol	2
University of Georgia	2
University of Kansas	2
University of Miami	2
University of Montreal	2
University of Stockholm	2
Veterans Admin. Hosp., Bronx, NY	2
Virginia Polytechnic Inst. and State University	2
Walter and Eliza Hall Inst. Med. Res., Melbourne, Australia	2
Wellcome Res. Lab., Kent, UK	2
Woods Hole Oceanographic Inst., Woods Hole, MA	2
Aarhus University, Risskov, Denmark	1
Abbott Labs., Chicago, IL	1
Academy of Natural Sciences, Philadelphia, PA	1
Academy of Sciences of the GDR	1
Allied Chemical Corp., Morristown, NJ	1
American Cyanamid Co., Stanford, CT	1
Argonne Natl. Lab., Argonne, IL	1
Australian Department of Science and Industrial Res.	1
Australian Natl. Observatory	1
Australian Natl. University	1
Bonneville Power Admin., Portland, OR	1
Bowling Green State University, OH	1
British Museum, London, UK	1
British Postgraduate Med. Sch., London, UK	1
Canadian Department of Fisheries	1
Cardiff Royal Infirmary, UK	1
Chester Beatty Res. Inst., London, UK	1
Children's Asthma Res. Inst. and Hosp., Denver, CO	1
Children's Hosp., Boston, MA	1
Clinical Res. Ctr., Harrow, UK	1
College of Veterinary Med., Finland	1
Crookes Labs., Ltd., London, UK	1
Defense Res. Board, Canada	1
Defense Res. Estab., Canada	1
Dennison University, Granville, OH	1
Distiller's Co., Epsom, UK	1
Eastman Kodak Co., Rochester, NY	1
Essex University, Colchester, UK	1
Fairchild Camera and Instrument Corp., Palo Alto, CA	1
Fels Res. Inst., Yellow Springs, OH	1
Free Hosp. for Women, Brookline, MA	1
Geophysics Corp. of America, Bedford, MA	1
Glynn Res. Labs., Bodmin, UK	1
Grasslands Res. Inst., Hurley, UK	1
Heyden Chemical Corp., Garfield, NJ	1
Hoffmann-La Roche, Nutley, NJ	1
Hosp. Infantil de Mexico, Mexico City, Mexico	1
Houghton Poultry Res. Station, Houghton, UK	1
Illinois Inst. Technology, Chicago, IL	1
Illinois Wesleyan University, Bloomington, IL	1
Imperial Cancer Res. Fund, London, UK	1
Imperial Chemical Indust., Welwyn Garden City, UK	1
Indian State Institution, New Delhi, India	1
Inst. Advanced Study, Princeton, NJ	1
Inst. Cancer Res., Philadelphia, PA	1
Inst. Fisheries Investigation, Spain	1
Inst. Psychiatric Res., Indianapolis, IN	1
Inst. Res. Cancer, Villejuif, France	1
Inst. Investigaciones Bioquimícas, Buenos Aires, Argentina	1
Jackson Lab., Bar Harbor, ME	1
Jewish Hosp., St. Louis, MO	1
Johannes Gutenberg University, Mainz	1
Kansas State University, Manhattan, KS	1
Karolinska Inst., Stockholm, Sweden	1
King Gustav V. Res. Inst., Stockholm, Sweden	1
École Normale Supérieure, Paris, France	1
Lund Inst. Technology, Sweden	1
M.D. Anderson Hosp. and Tumor Institution, Houston, TX	1
Martin Marietta Corp., Baltimore, MD	1
Massachusetts Mental Health Ctr., Boston, MA	1
Maudsley Hosp., London, UK	1
May Inst. Med. Res., Cincinnati, OH	1
Mead Johnson & Co., Evansville, IN	1
Medical Coll. Georgia, Augusta, GA	1
Methodist Hosp., Indianapolis, IN	1
Minnesota Mining & Manufacturing Co., St. Paul, MN	1
Mixing Equipment Co., Rochester, NY	1
Mobile Oil Co., Princeton, NJ	1
Natl. Inst. Mental Health, Rockville, MD	1
Natl. Vegetable Res. Station, Wellesbourne, UK	1
Natl. Women's Hosp., Auckland, New Zealand	1
New York Blood Center, NY	1
Nobel Medical Inst., Stockholm, Sweden	1
North Carolina State University	1
Oak Ridge Natl. Lab., TN	1
Ohio Agricultural Experimental Station, Columbus, OH	1
Ontario Cancer Inst.	1
Ontario Res. Foundation	1
Pneumoconiosis Res. Unit, Johannesburg, South Africa	1
Polytechnic Inst. New York, Farmingdale, NY	1
Portsmouth & Isle of Wight Area Pathology Serv., Portsmouth, UK	1
Quartermaster Res. and Engineering Ctr., Natick, MA	1
Research Board of Canada	1
Rijksuniversiteit Leiden, The Netherlands	1
Rockwell Park Memorial Inst., Buffalo, NY	1
Rowett Inst., Bucksburn, UK	1
Royal Coll. Science, London, UK	1
Royal Coll. Surgeons of England, London, UK	1

Royal Danish Sch. Pharmacy, Copenhagen, Denmark	1
Royal Radar Estab., Worcester, UK	1
Royal Signals & Radar Estab., Malvern, UK	1
Rutgers University, New Brunswick, NJ	1
St. Mark's Hosp., London, UK	1
Shell Development Co., Emeryville, CA	1
Sinclair Res. Labs., Harvey, IL	1
Sloan-Kettering Inst. Cancer Res., Rye, NY	1
Sorbonne, Paris, France	1
Squibb Inst. Med. Res., NJ	1
Standard Oil Co., Whiting, NJ	1
Swiss Federal Inst. Technology, Zurich, Switzerland	1
Technische Hochschule Stuttgart, FRG	1
Tohoku Pharmaceutical Sch., Sendai, Japan	1
Torry Res. Station, Aberdeen, UK	1
Tufts University	1
Tulane University of Louisiana	1
US Department of Commerce	1
US Department of Health, Education, and Welfare	1
US Department of Interior	1
US Geological Survey	1
University College, Swansea, UK	1
University College of Wales, UK	1
University of Aberdeen	1
University of Alaska	1
University of Arkansas	1
University of Bern, Switzerland	1
University of Bradford, UK	1
University of British Columbia	1
University of Cincinnati	1
University of Connecticut	1
University of Copenhagen	1
University of Durham	1
University of Düsseldorf	1
University of Florida	1
University of Freiburg	1
University of Geneva	1
University of Glasgow	1
University of Hawaii	1
University of Leeds	1
University of Liverpool	1
University of Louvain, Belgium	1
University of Manchester	1
University of Manitoba	1
University of Marburg	1
University of Maryland	1
University of Munich	1
University of Natal, South Africa	1
University of Nebraska	1
University of Newcastle-upon-Tyne	1
University of North Carolina	1
University of Ottawa	1
University of Sydney	1
University of Tennessee	1
University of Tokyo	1
University of Uppsala	1
University of Utah	1
University of Vermont	1
University of Waterloo, Canada	1
University of Western Ontario	1
Vanderbilt University	1
Varian Associates, Palo Alto, CA	1
Veterans Admin. Hosp., Durham, NC	1
Veterans Admin. Hosp., Little Rock, AR	1
Veterans Admin. Hosp., White River Junction, VT	1
Wake Forest University, Winston-Salem, NC	1
Westinghouse Electric Corp., Pittsburgh, PA	1
Westminster Hosp., London, UK	1
Wheeler Labs., Great Neck, NY	1
Wistar Inst., Philadelphia, PA	1
World Data Ctr., Moscow, USSR	1

Figure 3: *Citation Classics* authors' current institutional affiliations which were not listed in Figure 2. The number of authors from each is also shown.

Brigham Young University, Provo, UT	3
Tel Aviv University, Israel	3
Temple University, Philadelphia, PA	3
University of Arizona	3
University of Delaware	3
Brandeis University, Waltham, MA	2
CERN (European Organization for Nuclear Res.) Geneva, Switzerland	2
City University of New York	2
Colorado State University	2
Exxon Corp., Houston, TX	2
Hahnemann Med. Coll. and Hosp., Philadelphia, PA	2
University of Alberta, Edmonton, Canada	2
University of Guelph, Ontario, Canada	2
University of Oklahoma	2
University of Paris	2
University of Southampton	2
York University, Downsview, Ontario, Canada	2
Addenbrooke's Hosp., Cambridge, England	1
Aeronautical Res. Lab., Melbourne, Australia	1
All India Inst. Med. Sci., New Delhi, India	1
American University, Washington, DC	1
ARCO, Harvey, IL	1
Arizona State University	1
Bispebjerg Hosp., Copenhagen, Denmark	1
Bolt Beranek & Newman, Inc., Cambridge, MA	1
Boston University	1
Bruce Lyon Memorial Res. Lab., Oakland, CA	1
Brunel University, Uxbridge, UK	1
Brussels University, Belgium	1
CNRS, Strasbourg, France	1
California Department of Health	1
Canadian Wildlife Serv.	1
Cancer Control Agency of British Columbia, Vancouver, Canada	1
Cardiovascular and Chest Surgical Associates, Boise, ID	1
Carlsberg Foundation, Copenhagen, Denmark	1
Ctr. Studies of the Person, La Jolla, CA	1
Chalmers Inst. Technology, Göteborg, Sweden	1

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Children's Hosp. Med. Ctr., Cincinnati, OH	1
City of Hope Natl. Med. Ctr., Duarte, CA	1
Claremont Graduate Sch., CA	1
Clinical Res. Inst., Montreal, Canada	1
College of William & Mary	1
Communication Res. Ctr., Ottawa, Canada	1
Decision Res., Eugene, OR	1
Denver General Hosp.	1
Dickinson College, Carlisle, PA	1
Ecole National Supérieure des Mines, Paris, France	1
Ecology and Environment, Inc., Decatur, GA	1
Educational Testing Serv., Princeton, NJ	1
Florida Medical Entomology Lab., Vera Beach, FL	1
Freeman Hosp., Newcastle-upon-Tyne, UK	1
Funk Seeds Internatl., Bloomington, IL	1
Georgetown University, Washington, DC	1
Hampshire District Pathology Serv., Portsmouth, UK	1
Harris Corp., Melbourne, FL	1
Hazeltine Corp., Greenlawn, NY	1
Hôp. de l'Enfant Jesus, Quebec, Canada	1
Hughes Aircraft Co., Fullerton, CA	1
Hungarian Academy of Sciences, Budapest	1
Ibaraki University, Meto, Japan	1
Indian Inst. Science, Bangalore, India	1
Indiana Sch. Med.	1
Inst. Animal Physiology, Cambridge, UK	1
Inst. Biochemistry and Technology, Munster, FRG	1
Inst. Nacional de Ciencias y Technologia, Mexico	1
Intel Corp., Santa Clara, CA	1
International Lab. Res. Animal Diseases, Nairobi, Kenya	1
Israel Inst. Technology, Haifa	1
Justus-Liebig University, Geissen, FRG	1
LaTrobe University, Bundoora, Australia	1
Lehigh University, Bethlehem, PA	1
Letterman Army Inst. Res., San Francisco, CA	1
Lewisham Hospital, London, UK	1
Llandough Hosp., Penarth, UK	1
Loma Linda University Med. Ctr., CA	1
Long Island University, Greenvale, NY	1
Louisiana State University	1
Loyola University, Maywood, IL	1
McMaster University, Hamilton, Canada	1
Meat Res. Inst., Langford, UK	1
Michigan Technological University, Houghton, MI	1
Midwest Med. Lab., St. Louis, MO	1
Mill Hill Labs., London, UK	1
Mississippi State University	1
Monash University, Clayton, Australia	1
Montifiore Hosp. and Med. Ctr., New York, NY	1
Mt. Holyoke College, South Hadley, MA	1
Natl. Jewish Hosp. and Res. Ctr., Denver, CO	1
New York Med. Coll., NY	1
Oak Ridge Associated Universities, TN	1
Optical Sciences Co., Placentia, CA	1
Pace University, New York, NY	1
Peter MacCullum Hosp., Melbourne, Australia	1
Pittman-Moore, Glenorie, Australia	1
Polish Academy of Sciences, Warsaw, Poland	1
Portland State University	1
Queen Elizabeth Med. Ctr., Birmingham, UK	1
Queen's University, Kingston, Canada	1
Queen's University of Belfast, UK	1
Research & Laser Technology, Inc., Rockport, MA	1
Rheinische-Westfälische Technische Hochschule, Aachen, FRG	1
Rice University, Houston, TX	1
Royal Infirmary, Glasgow, UK	1
Royal Liverpool Hosp., Liverpool, UK	1
Rutgers Med. Sch., Piscataway, NJ	1
San Diego State University	1
Soroka Med. Ctr., Beer-Sheba, Israel	1
Southern Illinois University	1
Strangeways Res. Lab., Cambridge, UK	1
Swedish Department of Occupational Health	1
Syracuse University	1
Tokyo Inst. Technology, Japan	1
Tropical Products Institution, London, UK	1
US Food and Drug Admin.	1
USSR Academy of Science, Moscow	1
Uniformed Services University of the Health Services, Bethesda, MD	1
Unilever Res. Lab., Sharnbrook, UK	1
University of Alabama	1
University of Amsterdam	1
University of Auckland	1
University of Barcelona	1
University of Basel	1
University of Exeter	1
University of Göteborg, Sweden	1
University of Haifa, Israel	1
University of Iowa	1
University of Kentucky	1
University of Laval, Canada	1
University of Pierre and Marie Curie, Paris	1
University of Rhode Island	1
University of Saarland, FRG	1
University of Southern Alabama	1
University of Southern California	1
University of Sussex	1
University of Vienna	1
University of Virginia	1
University of Würzburg, FRG	1
Upjohn Co., Kalamazoo, MI	1
Varian/Extrio, Gloucester, MA	1
Veterans Admin. Ctr., Louisville, KY	1
Veterans Admin. Hosp., Miami, FL	1
Veterans Admin. Hosp., San Diego, CA	1
Washington State University, Pullman, WA	1
Western Michigan University	1
West Park Hosp., Epsom, UK	1
Wittenberg University, Springfield, OH	1
YMCA Tribal Development Project, Tamil Nadu, India	1
Retired	34

*One or more of these authors represents a second affiliation.

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ern US is significant. Also high on both lists are National Institutes of Health, 25 classics; University of Wisconsin, 24; Harvard, 22; Bell Labs., 18; University of London, 18; Johns Hopkins, 14; and Stanford, 13. Also prominent are Cornell, Columbia, University of Cambridge, University of Chicago, University of Pennsylvania, University of Washington, University of Texas, and State University of New York.

If we eliminate those institutions which account for only one paper, only 98 account for 523 classics. Similarly, only 105 institutions are involved in current affiliations. Seventy-three of these institutions appear on both lists. In Figure 4 there is a table showing the number of institutions which accounted for one or more classic papers.

Figure 4: The number of institutions which accounted for one or more classic papers.

Number of Papers	Number of Institutions
40	1
25	1
24	1
22	1
18	2
14	1
13	1
11	5
10	2
9	2
7	4
6	5
5	8
4	9
3	14
2	42
1	159

We also studied the number of authors per paper. It is significant that there are so many single-authored papers—302. Nevertheless, multi-authored papers dominate. Of these, however, 245 had only two authors. Papers in clinical medicine had an average of three authors, more authors than the other disciplines. We also know that high-energy physics papers have many authors. Figure 5 shows the number of papers for each group. The average number of authors per paper in the classics is 2.06. This is significantly lower (20 percent) than the 2.56 average for papers covered by the *Science Citation Index*® (*SCI*®) in 1980.

Which types of papers become *Citation Classics*? We know that superstar methodology papers, especially in biochemistry, are to be expected. Nevertheless, less than one fourth of the papers involved are methodological. Almost one third were theoretical or experimental. The remaining large category was review papers—about 100. And another large group was papers or books which provided oft-cited tables and data of one kind or another or "tests." These are only crude measures, but it should dispel the notion that methodology papers dominate citation studies. But once a well-known procedure is adopted it may be cited thousands of times. The interesting question is why a small number of such classics fail to succumb to the obliteration phenomenon.

Why are articles cited? The answer may seem, at first glance, quite simple, yet no one really knows—least of all the cited authors themselves! A surprising number of authors, upon receiving our request to comment upon their *Citation Classic*, have themselves questioned why their papers were cited. An equally large number of authors assert in their commentaries that they don't really know why they've been cited. It's amazing how many were not really aware how often or by whom they were cited. Only a small number are interested enough to go back to the citing papers through the *SCI* to find out exactly why they've been cited. In correspondence with some authors, I've pointed out that only a content analysis of the citing papers can reveal the "why" of citation.

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Figure 5: The number of authors per paper.

Number of Authors	Number of Papers
1	302
2	245
3	92
4	47
5	25
6	10
7	3
8	1
9	1
10	1
11	1

I also send them copies of a few ISI® papers in which we have done content analyses[9,10] and recommend they get students to help with the library work involved.

Of course, many authors maintain extensive reprint collections related to their discoveries or have an intense interest in the way their work has been applied. Hans Krebs, for example, in his *Citation Classic*,[11] quotes from an historical evaluation of his classic paper on urea formation. But most authors merely make educated guesses as to why they've been cited so often. I urge them to do these content analyses because they can lead to important review papers.

Authors frequently assert that timeliness was the main reason their papers had so much impact. Many feel that if their work had appeared earlier or later, it might not have received much notice. Others maintain that their papers were cited simply because they developed an often-used formula or procedure. Others believe that their papers were highly cited simply because they included a comprehensive review of the literature. But in my studies I have found that the influence of the *critical* review is significant—it doesn't simply serve as a convenient way to cite the earlier literature. Equally important, some of these authors correctly believe that the new or surprising or even the startling results published in their papers stimulated more work on the same topic. Sometimes the citing author makes this self-evident, but more often than not this is only implied in the work.

Most authors correctly assume that their papers were cited for positive reasons. Only a few authors feel that their work was also cited for negative reasons, although in at least one case, the paper was often cited for negative reasons.[12,13]

Understanding the "why" of citation is an area of research that interests sociologists of science. In fact, Susan Cozzens of ISI has recently summarized various theories about citations,[14] and in particular Mike Moravcsik, Oregon University, has examined the types of citations in physics in detail.[15] Their studies may lead to a better understanding of the processes of scientific discovery. In *The Force of Knowledge*, John Ziman, our salty colleague from the University of Bristol and editor of the *Philosophical Magazine* (a physics journal), points out the importance of citations to scientific knowledge:

> A typical scientific paper is full of references or citations to the experiments, calculations, observations, or theories of other people. It does not strike out on its own into the unknown, but timidly takes one little step forward from the base secured by previous research. In other words, modern research is highly collaborative, despite all the competition. Everything we do is deeply indebted to, and embedded in, the achievements of our predecessors and contemporaries in our Invisible College.[16] (p. 100-1)

In a 1971 article on citation indexes prepared by ISI for the *Encyclopedia of Library and Information Science*, we listed 15 reasons that authors cite other works. Among them were to pay homage, to identify methods or equipment, to provide background reading, and to give credit for related work.[17]

What some authors fail to recognize is that their "simple" discoveries make it possible for others to go on to do studies that were previously impossible. In evaluating the relative impact of highly cited papers the authors themselves can take the simplistic approach and assume that a paper was cited *simply* because a certain method or theory is used. But just as often the methodology is intertwined with other complex ideas that may never have been developed had not the methodology been employed. This was certainly the case recently when we examined why the work of R.M. Campbell *et al.* was cited.[18]

Perhaps the most fascinating aspect of *Citation Classics* is the perception of the author in evaluating the importance of the work in question. There is often a sense of irritation that we were not wise enough to realize that his or her most-cited work was not his or her most important. While we never make that assumption, it is somewhat distressing that we are resented because there is not a one to one correlation between citation frequency and the author's evaluation of importance. Clearly many authors feel, and correctly so, that the classic article we have selected *is* their most important work. But the opposite reaction of Heinz Fraenkel-Conrat and his coauthors is typical of many other classic authors. "All three authors have at least ten other papers to their credit which they would list above this one in importance. And what this paper is quoted for is not its intrinsic point (which had some importance) but for the fact that it contains a paragraph describing the method of washing and suspending commercial bentonite clay."[19] What Fraenkel-Conrat and others might have added is that some of their other articles were not only important papers but also qualify as *Citation Classics.*

Harriet Zuckerman, Columbia University, in her book *Scientific Elite*[20] discusses the phenomenon of the scientist who doesn't feel his highly acclaimed work is his most important. A significant number of Nobel prizewinners have these feelings. She offers some possible explanations. One, she says, is the fact that often the highly acclaimed work was the result of chance or serendipity. To some scientists, this seems less important, or valid, than something that was well thought-out or planned.[20]

Drawing on the work of Robert Merton, Columbia University, on the behavior of scientists, which demonstrated a common scientific drive for "recognized originality,"[21] Zuckerman further points out that since often an acclaimed work represents an accidental discovery, for many scientists this is not considered particularly original. Thus such work is not as meaningful as a work that would "deepen scientists' understanding of large problems."[20] (p. 211) Many Nobelists seem to feel that if it hadn't happened to them, it would have happened to someone else. We have here the idea that many discoveries are "inevitable." One wonders how much this kind of thinking pervades the halls of Congress these days.

Finally, says Zuckerman, there may often be a disparity between the personal significance of a work to its author, and the scientific significance of the same work,[22] that is, the acceptance of his or her peers. It's one thing to correlate an author's personal perception of importance to that of his or her peers and another to match this with actual impact as measured by citation frequency.

Still another explanation is offered by Lewis Goldberg, University of Oregon. In writing about his classic on the human judgment process, he notes that people frequently employ a technique he calls "availability" in decision mak-

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ing. "When people have to estimate the frequency of an event," he writes, "they typically rely on the ease with which instances spring to mind. Use of such a tactic is not unreasonable, but under some circumstances it can lead us astray (e.g. the frequency of more striking or memorable events gets overestimated). So it is with me: had someone asked me to estimate which of my publications was the most frequently cited, I'd certainly have selected another!"[23]

Taking a slightly different tack, Price hypothesizes that a failure of communication may actually be a major part of the problem. Many authors, he feels, are simply unable to evaluate their effectiveness at communication. An author, says Price, may well have discovered "the discovery to end all discoveries," but if he or she doesn't communicate that to others, it has little value. In other words, an author may be aware of the scientific quality of his or her work, but have no idea of the quality of communication. And, Price continues, citation frequency must be a product of the inherent quality of a work and the effectiveness of its communication.[8] He concludes that whether we like it or not, the citation record is the most accurate index of the world scientific community's opinion of a scientist we have available.[24]

The reader is, of course, the ultimate judge of whether the space and energy devoted to *Citation Classics* is worth the effort. I continue to read each and every classic before it is published. The quality has improved enormously over the years and in many instances we have been able to call attention to important work that has otherwise not been acknowledged. That many of the papers did elicit awards confirms our choice, but I take special satisfaction in serving as a public relations catalyst for many scientists who receive inadequate recognition for their work. Indeed, a large number of individuals turn up here who do not show up on our lists of most-cited authors. This illustrates that not only the superstars make important contributions to progress. I also hope it emphasizes the need for scientists and administrators to concentrate on publishing fewer papers of higher quality and hopefully higher impact.

Perhaps one of the most important contributions our *Citation Classics* series has made is that we now have extensive examples of significant discoveries that were entirely unplanned, and in fact, often accidental. This serendipitous theme is quite prevalent. As L. Révész of the Karolinska Institute observed: "Clearly, this is another example of how futile it is to try to foresee the path of fundamental research, to say nothing of governing it."[25] *Citation Classics* can serve as an excellent model of the actual discovery process. It is for this reason that we hope to publish them in a collection for use by graduate students and others.

In closing, may I urge every *CC* reader to inform us of any paper or book which may qualify as a *Citation Classic*. Each suggestion will be carefully evaluated and, if we agree, the authors will receive an invitation at the earliest opportunity. We are not referring here to unrecognized but important research. That is an entirely separate area of research which we are studying intensively. We will discuss these "sleepers" in the future. And when we complete the compilation of the *SCI* for 1955-1964, we will be better able to identify certain classics which very quickly after publication became a part of the common wisdom of science.

* * * * *

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Current Comments®

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The 1,000 Contemporary Scientists Most-Cited 1965-1978. Part I. The Basic List and Introduction

Number 41 October 12, 1981

At ISI® we've conducted hundreds of citation studies over the years. But none of them involved as massive an undertaking as that which preceded the compilation of the following list. (See Table 1.) As the title indicates, the list consists of the names of the 1,000 scientist-authors most-cited for work published from 1965-1978. This study took over two years to prepare, required approximately a month of large-scale computer time (over 750 hours), and drew upon the resources of many ISI staff members, especially Linda Cooper, manager of bibliographic research, editorial features. Others are acknowledged below.

We used "all-author" data derived from the *Science Citation Index®* (*SCI®*) data base, 1965-1978. Therefore, the list excludes social scientists. When we first began publishing lists of most-cited authors, we used first-author data only.[1] This was due to a continuing convention in the *Citation Index* section of *SCI*, in which cited and citing articles are listed under the name of only the first author listed on the paper. In the early days, these first-author lists were adequate for many purposes. But increasingly we felt the need to treat all authors in every article as though they were listed first. It is not just that science has become increasingly collaborative. Experienced scientists, already well known to their peers, were often and increasingly listed as secondary or coauthors. So we developed a system to let us do so.

In 1978 we used all-author data to publish a list of the 300 most-cited authors, 1961-1976.[2] The list which follows provides a new chronological perspective, although it is limited to the 14-year period, 1965-1978. This is an important four-year shift to even more contemporary science.

The list has also been expanded to consist of 1,000 scientists. Contrary to what I had expected, this step does not overcome a serious bias in this study and earlier ones. For a variety of reasons, the life sciences continue to be "overrepresented." Since citation practices vary so drastically among fields, caution must be used in the comparison of raw citation counts.[3] We have made no effort in this study to solve this problem by segmenting our files. This in fact will be done in follow-up studies to cover mathematics, earth sciences, physics, chemistry, and other fields outside the life sciences. It will become practical to do so as we implement new discipline-oriented data bases.

Developing all-author lists is considerably more complex than conducting first-author studies. To create this list, more than 67 million references in the *Citation Index* were compared with the entries in the *Source Index* of *SCI* for the period 1965-1978—more than five million published papers. It is not fully realized by most readers that the *Source Index* does in fact include every author of every article indexed—so we can treat each author as though he or she were the first author.

Table 1: The 1,000 most-cited authors for work published from 1965-1978, presented with their first and last names; field or discipline; birth and (where applicable) death dates; and number of citations received. An asterisk next to a name indicates possible incomplete information. A check mark indicates authors new to the top 300. Since citation practices vary among fields, caution should be used in comparing raw data.

AARONSON, STUART A	ONCOLOGY	1942	5072
* ABRAMS, GERALD STANLEY	PHYSICS	1941	3240
ADLER, STEPHEN L	PHYSICS	1939	3243
✓ AGHAJANIAN, GEORGE KEVORK	NEUROPHARMACOL	1932	4344
AKASOFU, SYUN ICHI	AERONOMY	1930	2536
✓ ALEXANDER, PETER	CELL BIOL	1922	4529
ALLEN, LELAND CULLEN	INORGANIC CHEM	1926	2621
ALLERHAND, ADAM	BIOPHYSICS	1937	2606
* ALLFREY, VINCENT GEORGE	BIOCHEMISTRY	1921	4166
* ALLINGER, NORMAN LOUIS	ORGANIC CHEM	1928	3023
ALLISON, ANTHONY CLIFFORD	IMMUNOLOGY	1925	6646
* ALPER, CHESTER A	IMMUNOLOGY	1931	2818
* ALTMAN, JOSEPH	NEUROLOGY	1925	2682
AMES, BRUCE NATHAN	ONCOLOGY	1928	4837
AMOS, DENNIS BERNARD	IMMUNOLOGY	1923	2660
ANDEN, NILS-ERIK BIRGER	PHARMACOLOGY	1937	5625
ANDERSON, PHILIP WARREN	PHYSICS	1923	2785
ANDREWS, PATRICK	BIOCHEMISTRY	1928	2515
ANFINSEN, CHRISTIAN BOEHMER	BIOCHEMISTRY	1916	4343
ANTONINI, ERALDO	BIOCHEMISTRY	1931	3127
AOKI, TADAO	IMMUNOLOGY	1930	2654
ARIAS, IRWIN MONROE	GASTROENTEROL	1926	3175
ARIMURA, AKIRA	ENDOCRINOLOGY	1923	[illegible]
ARNAUD, CLAUDE DONALD	ENDOCRINOLOGY	1929	2654
* ASKONAS, BRIGITTE ALICE	IMMUNOLOGY	1923	2542
ASOFSKY, RICHARD MARCY	IMMUNOLOGY	1933	2586
ATKINSON, DANIEL EDWARD	BIOCHEMISTRY	1921	3301
ATTARDI, GIUSEPPE	MOLEC BIOL	1926	3294
AURBACH, GERALD DONALD	ENDOCRINOLOGY	1927	5240
AUSTEN, KARL FRANK	IMMUNOLOGY	1929	6538
AUSTEN, WILLIAM GERALD	PHYSIOLOGY	1930	3702
AVIOLI, LOUIS V	ENDOCRINOLOGY	1931	2454
✓ AVRAMEAS, STRATIS	IMMUNOLOGY	1930	4717
AXELROD, JULIUS	PHARMACOLOGY	1912	12425
* AXEN, ROLF	BIOCHEMISTRY	1930	2573
✓ BACH, FRITZ H	IMMUNOLOGY	1934	4875
* BACH, JEAN FRANCOIS	IMMUNOLOGY	1940	2606
BAEHNER, ROBERT L	HEMATOLOGY	1934	2645
BAGLIONI, CORRADO	CELL BIOL	1933	2736
BAHCALL, JOHN NORRIS	ASTROPHYSICS	1934	2586
* BAKER, BERNARD RANDALL	ORGANIC CHEM	1915-76	2657
BALDWIN, ROBERT WILLIAM	ONCOLOGY	1927	2866
BALTIMORE, DAVID	VIROLOGY	1938	6773
BAME, SAMUEL JARVIS	ASTROPHYSICS	1924	3022
BARGER, VERNON DUANE	PHYSICS	1938	2613
* BARRELL, BARCLAY GEORGE	MOLEC BIOL	1944	2698
BARTON, DEREK HAROLD RICHARD	ORGANIC CHEM	1918	3177
* BARTTER, FREDERIC CROSBY	ENDOCRINOLOGY	1914	2616
BASERGA, RENATO	CELL BIOL	1925	3046
BASTEN, A	IMMUNOLOGY	1939	2790
* BATTERSBY, ALAN RUSHTON	ORGANIC CHEM	1925	[illegible]
BAULIEU, ETIENNE EMILE	ENDOCRINOLOGY	1926	2673
* BECKETT, ARNOLD HEYWORTH	PHARMACOLOGY	1920	2741
BENACERRAF, BARUJ	IMMUNOLOGY	1920	[illegible]
* BENDER, MYRON LEE	ORGANIC CHEM	1924	3118
BENESCH, REINHOLD	BIOCHEMISTRY	1919	3870
BENESCH, RUTH ERICA	BIOCHEMISTRY	1925	2742
BENSCH, KLAUS GEORGE	PATHOLOGY	1928	2507
BENSON, SIDNEY WILLIAM	PHYSICAL CHEM	1918	3334
BERG, PAUL	MOLEC BIOL	1926	3411
BERNARD, JEAN ALFRED	HEMATOLOGY	1907	2656
BERNARDI, GIORGIO	MOLEC BIOL	1929	2438
* BERNHARD, WILHELM	BIOPHYSICS	1920-78	2881
* BERNSTEIN, RICHARD BARRY	PHYSICAL CHEM	1923	3030
BEROZA, MORTON	ORGANIC CHEM	1917	3474
BERSON, SOLOMON A	ENDOCRINOLOGY	1918-72	3846
BESSER, GORDON MICHAEL	ENDOCRINOLOGY	1936	3567
BEUTLER, ERNEST	HEMATOLOGY	1928	4463
BIANCO, CELSO	IMMUNOLOGY	1941	2844
BIERMAN, EDWIN LAWRENCE	ENDOCRINOLOGY	1930	2606
* BIRNBAUMER, LUTZ	ENDOCRINOLOGY	1939	3196
BISHOP, JOHN MICHAEL	VIROLOGY	1936	3645
* BJORKEN, JAMES D	PHYSICS	1934	3196
BJORKLUND, ANDERS	CELL BIOL	1945	3549
* BLACK, PAUL H	VIROLOGY	1930	2680
BLANDEN, ROBERT VINCENT	IMMUNOLOGY	1938	2463
✓ BLOBEL, GUNTHER K J	CELL BIOL	1936	4050
BLOCH, KURT JULIUS	IMMUNOLOGY	1929	2881
BLOEMBERGEN, NICOLAAS	PHYSICS	1920	2428
✓ BLOOM, FLOYD ELLIOTT	NEUROPHARMACOL	1936	4898
* BLOOM, STEPHEN ROBERT	GASTROENTEROL	1942	3725
BLOW, DAVID MERVYN	BIOPHYSICS	1931	2485
BLUMBERG, BARUCH SAMUEL	ONCOLOGY	1925	5840
✓ BODEY, GERALD PAUL	ONCOLOGY	1934	4580
BOHLMANN, FERDINAND	ORGANIC CHEM	1921	2640
BONNER, JAMES FREDERICK	GENETICS	1910	7049
* BORISY, GARY GUY	MOLEC BIOL	1942	3110
BORNSTEIN, PAUL	CELL BIOL	1934	3071
BORSOS, TIBOR	IMMUNOLOGY	1927	3464
BORST, PIET	MOLEC BIOL	1934	2881
BOSMANN, HAROLD BRUCE	PHARMACOLOGY	1942	3196
BOWERS, CYRIL YARLING	ENDOCRINOLOGY	1924	3881
BOWIE, JOHN HAMILTON	ORGANIC CHEM	1938	2627
BOYARSKI, ADAM M	PHYSICS	1935	3356
BOYSE, EDWARD A	IMMUNOLOGY	1923	10159
BRADBURY, EDWIN MORTON	CELL BIOL	1933	2448
BRADY, ROSCOE OWEN	NEUROLOGY	1923	5086
* BRANTON, DANIEL	CELL BIOL	1932	3408
BRAUNWALD, EUGENE	CARDIOLOGY	1929	13483
BRAWERMAN, GEORGE	MOLEC BIOL	1927	2623
BREESE, GEORGE RICHARD	PHARMACOLOGY	1936	2973
BREIDENBACH, MARTIN	PHYSICS	1943	3786
BRENNER, SYDNEY	MOLEC BIOL	1927	2811
✓ BRITTEN, ROY J	MOLEC BIOL	1919	4380
BRODIE, BERNARD BERYL	PHARMACOLOGY	1909	5680
BROWN, DONALD D	MOLEC BIOL	1931	3219
BROWN, GERALD E	PHYSICS	1926	2957
BROWN, HERBERT C	ORGANIC CHEM	1912	8766
BROWN, JEHOIADA J	PHYSIOLOGY	1927	3906
* BROWN, MICHAEL S	BIOCHEMISTRY	1941	3180
BROWNLEE, G G	MOLEC BIOL	1942	2982
BRUICE, THOMAS CHARLES	ORGANIC CHEM	1925	2876
BRUNNER, HANS RUDOLF	CARDIOLOGY	1937	2632
BRUNNER, K T	IMMUNOLOGY	1916	2617
* BULOS, FAGIN	PHYSICS	1925	3008
BUNNEY, WILLIAM EDWARD	PSYCHIATRY	1930	2743
* BURCH, GEORGE E	CARDIOLOGY	1910	2786
✓ BURGER, MAX MARCEL	CELL BIOL	1933	4443
BURGUS, ROGER CECIL	ENDOCRINOLOGY	1934	2962
BURNSTOCK, GEOFFREY	PHARMACOLOGY	1929	3080
BUSCH, HARRIS	PHARMACOLOGY	1923	4621
* BUTCHER, REGINALD WILLIAM	PHYSIOLOGY	1930	5511
BUTLER, WARREN LEE	BIOPHYSICS	1925	2494
CAHILL, GEORGE FRANCIS	PHYSIOLOGY	1927	3783
CAMERON, ALASTAIR GRAHAM W	ASTROPHYSICS	1925	2626
* CANELLOS, GEORGE P	ONCOLOGY	1934	2453
CANTOR, CHARLES ROBERT	BIOCHEMISTRY	1942	2616
CANTOR, HARVEY	IMMUNOLOGY	1942	2506
CARBONE, PAUL P	ONCOLOGY	1931	4662
CARDONA, MANUEL	PHYSICS	1936	3180
CARLSON, LARS A	PHYSIOLOGY	1928	3374
CARLSSON, ARVID	NEUROPHARMACOL	1923	4118
CASIDA, JOHN EDWARD	BIOCHEMISTRY	1929	2445
CASPERSSON, TORBJORN O	CELL BIOL	1910	3580
* CATT, KEVIN JOHN	ENDOCRINOLOGY	1932	3782
CEROTTINI, JEAN-CHARLES	IMMUNOLOGY	1938	3100
CHALKLEY, ROGER	MOLEC BIOL	1938	3742
CHAMBON, PIERRE H	MOLEC BIOL	1931	3367
* CHANCE, BRITTON	BIOCHEMISTRY	1913	7131
CHANGEUX, JEAN-PIERRE	NEUROLOGY	1936	6701
* CHANOCK, ROBERT MERRITT	VIROLOGY	1924	6080
✓ CHAPMAN, DENNIS	BIOPHYSICS	1927	4404
CHATT, JOSEPH	INORGANIC CHEM	1914	2886
* CHINOWSKY, WILLIAM	PHYSICS	1929	2965
* CHOPPIN, PURNELL WHITTINGTON	VIROLOGY	1929	3098
* CHOPRA, INDER JIT	ENDOCRINOLOGY	1939	2632
CHRAMBACH, ANDREAS C	BIOCHEMISTRY	1927	2744
CHURCHILL, MELVYN ROWEN	INORGANIC CHEM	1940	3663
CLAMAN, HENRY NEUMANN	IMMUNOLOGY	1930	3076
* CLARK, ALVIN JOHN	MOLEC BIOL	1933	2662
CLARK, HOWARD CHARLES	ORGANOMET CHEM	1929	3036
CLARK, ROBIN JON HAWES	INORGANIC CHEM	1935	2506
CLEAVER, JAMES EDWARD	GENETICS	1938	3206
CLEGG, JOHN BRIAN	HEMATOLOGY	1936	2960
* CLEMENTI, ENRICO	THEORETICAL CHEM	1931	4601
* CLIFFORD, PETER	ONCOLOGY	1921	3444
CLINE, MARTIN J	HEMATOLOGY	1934	3866
COBURN, JACK WESLEY	PHYSIOLOGY	1932	2626
COCHRANE, CHARLES G	IMMUNOLOGY	1930	3648
COGGESHALL, RICHARD E	NEUROLOGY	1932	3766
COHEN, ALAN SEYMOUR	PATHOLOGY	1926	2749
✓ COHEN, MARVIN LOU	PHYSICS	1935	2662
COHEN, STANLEY N	MOLEC BIOL	1935	2964
* COHN, MELVIN	IMMUNOLOGY	1922	2740
✓ COHN, ZANVIL A	CELL BIOL	1926	4182
COLLMAN, JAMES PADDOCK	INORGANIC CHEM	1932	3188
COMINGS, DAVID EDWARD	CELL BIOL	1935	2987
CONN, JEROME W	ENDOCRINOLOGY	1907	2944
CONNEY, ALLAN HOWARD	PHARMACOLOGY	1930	7866
COOKS, ROBERT GRAHAM	ORGANIC CHEM	1941	2564
COOLEY, DENTON ARTHUR	CARDIOLOGY	1920	2866
COON, MINOR J	ENZYMOLOGY	1921	2647
COOPER, MAX DALE	IMMUNOLOGY	1933	5774
COREY, ELIAS JAMES	ORGANIC CHEM	1928	9152
CORRODI, HANS	PHARMACOLOGY	1929-74	5010
COSTA, ERMINIO	PHARMACOLOGY	1924	6420
COTTON, FRANK ALBERT	INORGANIC CHEM	1930	9846
✓ COVELL, JAMES WACHOB	PHYSIOLOGY	1936	4578
COWAN, WILLIAM MAXWELL	NEUROLOGY	1931	3150
COWLEY, ROGER ARTHUR	PHYSICS	1939	2986
COY, DAVID HOWARD	ENDOCRINOLOGY	1944	2886
CRAM, DONALD JAMES	ORGANIC CHEM	1919	2648
CROMER, DON T	PHYSICS	1923	6776
CUATRECASAS, PEDRO	BIOCHEMISTRY	1936	17643
* CURRAN, PETER FERGUSON	BIOPHYSICS	1931-74	2861
CURTIS, DAVID RODERICK	PHARMACOLOGY	1927	3816
* DAHL, LAWRENCE FREDERICK	PHYSICAL CHEM	1929	3440
DAHLSTROM, ANNICA B	HISTOLOGY	1941	2513
DALGARNO, ALEXANDER	ASTROPHYSICS	1928	4127
✓ DALY, JOHN W	PHARMACOLOGY	1933	6606
✓ DAMATO, ANTHONY NICHOLAS	CARDIOLOGY	1930	4581
DARNELL, JAMES EDWIN	CELL BIOL	1930	7604
* DATTA, NAOMI	MOLEC BIOL	1922	2796
DAUGHADAY, WILLIAM HAMILTON	ENDOCRINOLOGY	1918	3573
DAUSSET, JEAN	IMMUNOLOGY	1916	2661

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✓ DAVID, JOHN R.	IMMUNOLOGY	1930	4243
DAVIDSON, ERIC HARRIS	MOLEC BIOL	1937	3534
DAVIDSON, ERNEST ROY	PHYSICAL CHEM	1936	5123
✓ DAVIDSON, NORMAN RALPH	MOLEC BIOL	1916	4702
DAVIS, JAMES OTHELLO	PHYSIOLOGY	1916	2463
DAVIS, JOHN MARCELL	PSYCHIATRY	1933	3482
DAWSON, R.M.C.	BIOCHEMISTRY	1924	2477
DE DUVE, CHRISTIAN RENE	CELL BIOL	1917	4863
DE ROBERTIS, EDUARDO DIEGO P.	CELL BIOL	1913	2609
✓ DE VITA, VINCENT T.	ONCOLOGY	1935	5163
DELUCA, HECTOR FLOYD	BIOCHEMISTRY	1930	13080
DEWAR, MICHAEL JAMES STEUART	PHYSICAL CHEM	1918	8388
* DIAMOND, JARED MASON	PHYSIOLOGY	1937	2845
DICZFALUSY, EGON	ENDOCRINOLOGY	1920	2758
DIETSCHY, JOHN MAURICE	GASTROENTEROL	1932	2504
DIXON, FRANK JAMES	IMMUNOLOGY	1920	7458
DJERASSI, CARL	ORGANIC CHEM	1923	7704
* DOLL, RICHARD	ONCOLOGY	1912	2698
* DOLLERY, COLIN TERENCE	PHARMACOLOGY	1931	3016
DOTY, PAUL MEAD	MOLEC BIOL	1920	2782
DOUGLAS, STEVEN DANIEL	IMMUNOLOGY	1939	3098
DOWLING, JOHN ELLIOTT	PHYSIOLOGY	1935	2865
DRAGO, RUSSELL STEPHEN	INORGANIC CHEM	1928	3402
DRAY, SHELDON	IMMUNOLOGY	1920	2784
DRELL, SIDNEY DAVID	PHYSICS	1926	2749
DRUCKREY, HERMANN	ONCOLOGY	1904	2567
* DUBOIS, JACQUES EMILE	PHYSICAL CHEM	1920	2798
DUDRICK, STANLEY J.	SURGERY	1935	2535
* DUESBERG, PETER H.	VIROLOGY	1936	3071
DUKE, CHARLES BRYAN	PHYSICS	1938	2985
DULBECCO, RENATO	ONCOLOGY	1914	2921
DURIG, JAMES ROBERT	INORGANIC CHEM	1935	3136
DUSTAN, HARRIET PEARSON	CARDIOLOGY	1920	2490
DUTTON, RICHARD WILLIAM	IMMUNOLOGY	1930	3058
EASTMAN, DEAN E.	PHYSICS	1940	2891
EDELHOCH, HAROLD	BIOCHEMISTRY	1922	2944
* EDELMAN, GERALD MAURICE	IMMUNOLOGY	1929	7675
ELIEL, ERNEST LUDWIG	ORGANIC CHEM	1921	2981
✓ ENGEL, WILLIAM KING	NEUROLOGY	1930	4031
EPSTEIN, STEPHEN E.	CARDIOLOGY	1936	6243
* ERICSSON, JAN LARS-ERIK	PATHOLOGY	1932	3055
* ERNSTER, LARS	BIOCHEMISTRY	1920	3060
ESTABROOK, RONALD WINFIELD	BIOCHEMISTRY	1926	4314
EXTON, JOHN H.	ENDOCRINOLOGY	1933	3149
* EYLAR, EDWIN HAROLD	BIOCHEMISTRY	1934	3293
FAHEY, JOHN LESLIE	IMMUNOLOGY	1924	4283
FAIRBANKS, GRANT	CELL BIOL	1940	3210
FALCK, BENGT O.F.	HISTOLOGY	1927	2721
FARBER, EMMANUEL	PATHOLOGY	1918	2675
FARQUHAR, MARILYN GIST	CELL BIOL	1928	3612
FASMAN, GERALD DAVID	BIOCHEMISTRY	1925	4536
* FAWCETT, DON WAYNE	HISTOLOGY	1917	2472
FEFER, ALEXANDER	IMMUNOLOGY	1936	2775
FEHSENFELD, FRED C.	GEOPHYSICS	1934	2882
FEIGENBAUM, HARVEY	CARDIOLOGY	1933	3123
FELDMAN, GARY JAY	PHYSICS	1942	3452
FELDMANN, MARC	IMMUNOLOGY	1944	3750
✓ FELIG, PHILIP	ENDOCRINOLOGY	1936	3861
FELSENFELD, GARY	MOLEC BIOL	1929	3030
* FERGUSON, ELDON EARL	AERONOMY	1929	3446
FIELD, EPHRAIM JOSHUA	NEUROLOGY	1915	2679
FINCH, CLEMENT ALFRED	HEMATOLOGY	1915	2851
FINLAND, MAXWELL	MICROBIOL	1902	3252
FISCHER, ERNST OTTO	ORGANOMET CHEM	1918	3472
FISHER, BERNARD	ONCOLOGY	1918	2764
FISHER, DELBERT A.	ENDOCRINOLOGY	1928	3479
* FISHER, EDWIN RALPH	PATHOLOGY	1923	2864
FISHER, MICHAEL ELLIS	PHYSICS	1931	4624
FISHMAN, WILLIAM HAROLD	ONCOLOGY	1914	2628
FLEISHER, SIDNEY	MOLEC BIOL	1930	2625
FLORY, PAUL JOHN	PHYSICAL CHEM	1910	5057
FLYGARE, WILLIS H.	CHEM PHYS	1936	2790
* FOLKERS, KARL AUGUST	ORGANIC CHEM	1906	2886
FORSHAM, PETER HUGH	ENDOCRINOLOGY	1915	3446
FOUTS, JAMES RALPH	PHARMACOLOGY	1929	2584
FRANK, MICHAEL M.	IMMUNOLOGY	1937	2888
FRANKE, WERNER W.	CELL BIOL	1940	3051
FRANKLIN, EDWARD CLAUS	IMMUNOLOGY	1928	2684
FREDRICKSON, DONALD SHARP	GENETICS	1924	6409
FREEDMAN, SAMUEL ORKIN	IMMUNOLOGY	1928	3146
* FREI, EMIL	ONCOLOGY	1924	3609
FREIREICH, EMIL J	ONCOLOGY	1927	4736
✓ FRIDOVICH, IRWIN	ENZYMOLOGY	1929	5141
FRIEDBERG, CARL E.	PHYSICS		2984
* FRIEDMAN, ROBERT MORRIS	VIROLOGY	1932	3164
FRIESEN, HENRY GEORGE	ENDOCRINOLOGY	1934	2645
FRYBERGER, DAVID	PHYSICS	1931	2851
FUDENBERG, H. HUGH	IMMUNOLOGY	1928	9602
FUXE, KJELL G.	CELL BIOL	1938	13219
GALL, JOSEPH GRAFTON	MOLEC BIOL	1928	3465
✓ GALLO, ROBERT C.	CELL BIOL	1937	4140
GARATTINI, SILVIO	PHARMACOLOGY	1928	2830
GARITO, ANTHONY FRANK	PHYSICS	1939	2836
GASSMAN, PAUL G.	ORGANIC CHEM	1935	2887
✓ * GELBOIN, HARRY VICTOR	BIOCHEMISTRY	1929	4106
GERGELY, JOHN	BIOCHEMISTRY	1919	2465
GERSHON, RICHARD K.	IMMUNOLOGY	1932	2748
GEWURZ, HENRY	IMMUNOLOGY	1936	3820
GIACCONI, RICCARDO	ASTRONOMY	1931	2967
GIACOMELLI, GIORGIO M.	PHYSICS	1931	2463
GIBBONS, RONALD JOHN	MICROBIOL	1932	2447
GIBLETT, ELOISE ROSALIE	HEMATOLOGY	1921	2447
* GIBSON, QUENTIN HOWIESON	PHYSIOLOGY	1918	2529
✓ * GILDEN, RAYMOND VICTOR	IMMUNOLOGY	1935	3391
* GILLESPIE, DAVID H.	GENETICS	1940	2703
GILLETTE, JAMES ROBERT	PHARMACOLOGY	1928	4857
✓ GILMAN, ALFRED G.	PHARMACOLOGY	1941	3867
* GLASHOW, SHELDON LEE	PHYSICS	1932	3539
GLOWINSKI, J.	NEUROPHARMACOL	1936	6362
✓ GOLD, PHIL	ONCOLOGY	1936	2958
GOLDBERG, NELSON D.	PHARMACOLOGY	1931	2461
✓ GOLDHABER, GERSON	PHYSICS	1924	2855
GOLDSTEIN, AVRAM	PHARMACOLOGY	1919	2524
GOLDSTEIN, IRWIN JOSEPH	BIOCHEMISTRY	1929	3201
GOLDSTEIN, JOSEPH LEONARD	GENETICS	1940	3666
GOLDSTEIN, MENEK	NEUROLOGY	1924	2577
GOOD, ROBERT ALAN	IMMUNOLOGY	1922	17679
GOODMAN, HOWARD MICHAEL	MOLEC BIOL	1938	2839
GOODWIN, FREDERICK KING	PSYCHIATRY	1936	2796
GOODWIN, TREVOR WALWORTH	BIOCHEMISTRY	1916	2543
GORBACH, SHERWOOD LESLIE	MICROBIOL	1934	3460
GORDEN, PHILLIP	ENDOCRINOLOGY	1934	2503
* GORDON, ROY GERALD	PHYSICAL CHEM	1940	3087
* GORLIN, RICHARD	CARDIOLOGY	1926	5864
GORSKI, JACK	ENDOCRINOLOGY	1931	3225
GRAHAM, RICHARD CYRIL	IMMUNOLOGY	1934	3322
GRANT, DAVID MORRIS	PHYSICAL CHEM	1931	4246
GRAY, HARRY B.	INORGANIC CHEM	1935	4627
GREAVES, MELVYN FRANCIS	IMMUNOLOGY	1941	3457
GREEN, DAVID EZRA	BIOPHYSICS	1910	2807
GREEN, HOWARD	CELL BIOL	1925	4329
✓ GREEN, IRA	IMMUNOLOGY	1926	4329
GREEN, MAURICE	MOLEC BIOL	1926	3664
* GREEN, MICHAEL	INORGANIC CHEM	1934	2629
GREENGARD, PAUL	CELL BIOL	1925	6666
GREINER, WALTER A.	PHYSICS	1935	3299
* GREY, HOWARD M.	PATHOLOGY	1932	4645
GRODSKY, GEROLD MORTON	ENDOCRINOLOGY	1927	2781
* GROS, FRANCOIS	MOLEC BIOL	1925	2491
GROSS, DAVID JONATHAN	PHYSICS	1941	2884
* GROSS, JEROME	BIOCHEMISTRY	1917	2546
GROSSMAN, MORTON IRVIN	PHYSIOLOGY	1919	6663
GROVER, PHILIP L.	ONCOLOGY	1933	3158
GRUMBACH, MELVIN MALCOLM	ENDOCRINOLOGY	1925	3667
GUGGENHEIM, HOWARD J.	INORGANIC CHEM	1923	2573
GUILLEMIN, ROGER	ENDOCRINOLOGY	1924	3694
GURSKY, HERBERT	ASTRONOMY	1930	3335
HABER, EDGAR	IMMUNOLOGY	1932	5699
HAGEMAN, RICHARD HARRY	PLANT SCIENCES	1917	2837
HAKANSON, ROLF	PHARMACOLOGY	1937	3669
HALL, REGINALD	ENDOCRINOLOGY	1931	3132
* HALPERN, JACK	INORGANIC CHEM	1925	2572
✓ HAMBERG, MATS	BIOCHEMISTRY	1944	4615
HAMILTON, WALTER CLARK	PHYSICAL CHEM	1931-73	3915
HAMMOND, GEORGE SIMMS	ORGANIC CHEM	1921	3556
✓ HANSCH, CORWIN H.	ORGANIC CHEM	1918	3625
HANSEN, OLE	PHYSICS	1934	3409
HANSON, GAIL G.	PHYSICS	1947	3242
HARARI, H.	PHYSICS	1940	2492
HARDMAN, JOEL GRIFFETH	PHARMACOLOGY	1933	2873
HARRIS, HARRY	GENETICS	1919	2725
HARRIS, HENRY	CELL BIOL	1925	3981
* HARRISON, DONALD C.	CARDIOLOGY	1934	4189
HARTLEY, BRIAN SELBY	MOLEC BIOL	1926	3382
✓ * HARTLEY, JANET WILSON	VIROLOGY	1929	4026
* HASZELDINE, ROBERT NEVILLE	ORGANOMET CHEM	192[illegible]	2954
HAWTHORNE, MARION FREDERICK	INORGANIC CHEM	1928	2749
HAYAISHI, OSAMU	ENZYMOLOGY	1920	3437
HAYFLICK, LEONARD	CELL BIOL	1928	3534
* HAYON, ELIE	PHYSICAL CHEM	1932	2529
HEEGER, ALAN J.	PHYSICS	1936	3490
✓ HEHRE, WARREN J.	THEORETICAL CHEM	1945	4656
HEIDELBERGER, CHARLES	ONCOLOGY	1920	4214
* HEILBRONNER, EDGAR	ORGANIC CHEM	1921	3196
HELFANT, RICHARD H.	CARDIOLOGY	1937	2464
HELINSKI, DONALD RAYMOND	GENETICS	1933	3036
* HELLMAN, LEON	ONCOLOGY	1921	3112
HELLSTROM, INGEGERD E.	IMMUNOLOGY	1932	6062
HELLSTROM, KARL ERIK	IMMUNOLOGY	1934	6145
* HENDERSON, EDWARD S.	ONCOLOGY	1932	3688
HENDERSON, JOSEPH F.	BIOCHEMISTRY	1933	2665
* HENLE, GERTRUDE	VIROLOGY	1912	6818
* HENLE, WERNER	VIROLOGY	1910	6233
HENNEY, CHRISTOPHER SCOT	IMMUNOLOGY	1941	2732
✓ HERBERMAN, RONALD B.	ONCOLOGY	1940	5087
HERBERT, VICTOR DANIEL	HEMATOLOGY	1927	4716
HEREMANS, JOSEPH FELIX	IMMUNOLOGY	1927-75	3524
✓ HERSH, EVAN MANUEL	IMMUNOLOGY	1935	4324
HERZENBERG, LEONARD ARTHUR	IMMUNOLOGY	1931	3888
HILL, ROBERT LEE	BIOCHEMISTRY	1928	2916
* HILLEMAN, MAURICE RALPH	VIROLOGY	1919	3447
HIRSCH, JAMES GERALD	CELL BIOL	1922	2653
HIRSCHHORN, KURT	GENETICS	1926	3070
HOBBS, JOHN RAYMOND	PATHOLOGY	1929	2768
HOFFBRAND, ALLAN VICTOR	HEMATOLOGY	1935	3598
HOFFMAN, BRIAN F.	CARDIOLOGY	1925	3686
HOFFMANN, ROALD	INORGANIC CHEM	1937	7340
HOFMANN, ALAN F.	GASTROENTEROL	1931	4863
* HOKFELT, TOMAS	NEUROPHARMACOL	1940	6666
HOLBOROW, ERIC JOHN	IMMUNOLOGY	1916	3314
HOLLAND, JAMES FREDERICK	ONCOLOGY	1925	3743
HOLM, OLOF GORAN	HEMATOLOGY	1930	3207
HOLM, RICHARD HADLEY	INORGANIC CHEM	1934	2984
HOLTZER, HOWARD	CELL BIOL	1922	3088
HONG, RICHARD	IMMUNOLOGY	1929	2661
HORECKER, BERNARD LEONARD	BIOCHEMISTRY	1914	3160
HOUSE, HERBERT O.	ORGANIC CHEM	1929	2214
HSU, TAO-CHIUH	GENETICS	1917	2620
HUBEL, DAVID HUNTER	PHYSIOLOGY	1926	3784
HUEBNER, ROBERT JOSEPH	ONCOLOGY	1914	7526
HUISGEN, ROLF K.J.	ORGANIC CHEM	1920	5467
* HUISMAN, TITUS HENDRIK JAN	BIOCHEMISTRY	1923	3092

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HURWITZ, JERARD	MOLEC BIOL	[illegible]	[illegible]
IBEN, ICKO	ASTROPHYSICS	1931	[illegible]
IBERS, JAMES ARTHUR	INORGANIC CHEM	[illegible]	7575
INBAR, MICHAEL	CELL BIOL	[illegible]	3577
INGBAR, SIDNEY HAROLD	ENDOCRINOLOGY	[illegible]	[illegible]
INGOLD, KEITH U.	ORGANIC CHEM	[illegible]	[illegible]
ISHIZAKA, KIMISHIGE	IMMUNOLOGY	[illegible]	5102
ISHIZAKA, TERUKO	IMMUNOLOGY	[illegible]	[illegible]
ISSELBACHER, KURT JULIUS	GASTROENTEROL	[illegible]	4771
IVANKOVIC, S.	ONCOLOGY	1931	[illegible]
IVERSEN, LESLIE L.	PHARMACOLOGY	1937	[illegible]
IZAWA, SEIKICHI	PLANT SCIENCES	[illegible]	[illegible]
JACKIW, ROMAN WLADIMIR	PHYSICS	[illegible]	[illegible]
JACKSON, RICHARD LEE	BIOCHEMISTRY	[illegible]	[illegible]
JACOB, FRANCOIS	MOLEC BIOL	1920	[illegible]
JAMES, THOMAS NAUM	CARDIOLOGY	[illegible]	[illegible]
JEAN-MARIE, BERNARD	PHYSICS	1940	[illegible]
JENKINS, WILLIAM PLATT	ORGANIC CHEM	1927	[illegible]
• JERINA, DONALD M.	ORGANIC CHEM	1940	[illegible]
JOHANSSON, S.G.O.	IMMUNOLOGY	[illegible]	[illegible]
• JOHNSON, BRIAN F.G.	ORGANIC CHEM	[illegible]	[illegible]
JOHNSON, GEORGE S.	BIOCHEMISTRY	1943	[illegible]
JOHNSON, LEONARD ROY	PHYSIOLOGY	1942	[illegible]
JOHNSTON, GRAHAM ALLEN	PHARMACOLOGY	[illegible]	[illegible]
JOKLIK, WOLFGANG KARL	VIROLOGY	[illegible]	[illegible]
JONDAL, MIKAEL B.	IMMUNOLOGY	1943	[illegible]
• JONES, EDWARD GEORGE	NEUROBIOLOGY	[illegible]	[illegible]
JONSSON, GOSTA LARS	HISTOLOGY	1941	[illegible]
JORTNER, JOSHUA	PHYSICS	1933	[illegible]
JOUVET, MICHEL	PHYSIOLOGY	[illegible]	3137
KABACK, HOWARD RONALD	MOLEC BIOL	[illegible]	[illegible]
KABAT, ELVIN ABRAHAM	IMMUNOLOGY	1914	[illegible]
KADYK, JOHN AMOS	PHYSICS	[illegible]	[illegible]
KAKKAR, V.V.	CARDIOLOGY	[illegible]	[illegible]
• KAMETANI, TETSUJI	PHARMACOLOGY	1917	3741
KANNEL, WILLIAM B.	CARDIOLOGY	[illegible]	[illegible]
KAPLAN, HENRY SEYMOUR	ONCOLOGY	1918	[illegible]
KAPLAN, NATHAN ORAM	ENZYMOLOGY	1917	[illegible]
KAPLAN, SELNA L.	ENDOCRINOLOGY	1927	[illegible]
KAPPAS, ATTALLAH	PHARMACOLOGY	[illegible]	[illegible]
KARLE, ISABELLA LUGOSKI	BIOPHYSICS	1921	[illegible]
KARNOVSKY, MORRIS JOHN	CELL BIOL	[illegible]	11427
• KARPLUS, MARTIN	PHYSICAL CHEM	1930	3777
KASTIN, ABBA J.	ENDOCRINOLOGY	1934	[illegible]
KATCHALSKI-KATZIR, EPHRAIM	BIOPHYSICS	1916	[illegible]
KATO, RYUICHI	PHARMACOLOGY	[illegible]	[illegible]
KATRITZKY, ALAN ROY	ORGANIC CHEM	[illegible]	[illegible]
KATZ, BERNARD	BIOPHYSICS	1911	[illegible]
KATZ, DAVID H.	IMMUNOLOGY	1943	[illegible]
• KAUFMAN, SEYMOUR	ENZYMOLOGY	[illegible]	[illegible]
KEARNS, DAVID R.	BIOPHYSICS	[illegible]	[illegible]
KELLEY, WILLIAM NIMMONS	GENETICS	[illegible]	[illegible]
KHORANA, HAR GOBIND	ORGANIC CHEM	1922	[illegible]
• KING, ROBERT BRUCE	ORGANOMET CHEM	[illegible]	[illegible]
KIPNIS, DAVID MORRIS	CELL BIOL	1927	[illegible]
KIRBY, WILLIAM M.M.	MICROBIOL	1914	[illegible]
KIVIRIKKO, KARI ILKKA	BIOCHEMISTRY	1937	[illegible]
KLEBANOFF, SEYMOUR JOSEPH	IMMUNOLOGY	1927	[illegible]
KLEEMAN, CHARLES RICHARD	PHYSIOLOGY	[illegible]	[illegible]
KLEIN, EVA	IMMUNOLOGY	[illegible]	[illegible]
KLEIN, GEORGE	IMMUNOLOGY	[illegible]	10947
KLEIN, JAN	GENETICS	[illegible]	[illegible]
KLINGENBERG, MARTIN ERNST	BIOPHYSICS	[illegible]	[illegible]
KLUG, AARON	MOLEC BIOL	[illegible]	[illegible]
• KNOBIL, ERNST	ENDOCRINOLOGY	[illegible]	[illegible]
KOCH-WESER, JAN	PHARMACOLOGY	1927	[illegible]
KOCHI, JAY KAZUO	ORGANOMET CHEM	[illegible]	[illegible]
KODICEK, EGON HYNEK	BIOCHEMISTRY	[illegible]	[illegible]
KOPIN, IRWIN J.	PHARMACOLOGY	[illegible]	[illegible]
• KOPROWSKI, HILARY	MICROBIOL	1916	[illegible]
KORN, EDWARD DAVID	CELL BIOL	[illegible]	[illegible]
KORNBERG, ARTHUR	MOLEC BIOL	1918	[illegible]
• KOSHLAND, DANIEL EDWARD	BIOCHEMISTRY	1920	[illegible]
KREBS, EDWIN GERHARD	BIOCHEMISTRY	1918	[illegible]
KREBS, HANS ADOLF	BIOCHEMISTRY	1900	[illegible]
KRISHNA, GOPAL AIYER	PHARMACOLOGY	[illegible]	[illegible]
KRNJEVIC, KRESIMIR	PHYSIOLOGY	1927	[illegible]
KRUGMAN, SAUL	VIROLOGY	1911	[illegible]
KUHAR, MICHAEL JOSEPH	NEUROPHARMACOL	1944	[illegible]
KUNKEL, HENRY GEORGE	IMMUNOLOGY	[illegible]	9134
KUNTZMAN, RONALD GROVER	PHARMACOLOGY	[illegible]	[illegible]
KUO, JYH-FA	PHARMACOLOGY	[illegible]	[illegible]
• KUPCHAN, S. MORRIS	ORGANIC CHEM	[illegible]-76	[illegible]
KURLAND, C.G.	MOLEC BIOL	[illegible]	[illegible]
LACY, PAUL ESTON	PATHOLOGY	[illegible]	[illegible]
LAEMMLI, U.K.	MOLEC BIOL	1940	[illegible]
• LANDON, J.	PATHOLOGY	[illegible]	2712
• LANDS, WILLIAM EDWARD M.	BIOCHEMISTRY	[illegible]	[illegible]
LAPPERT, MICHAEL FRANZ	ORGANOMET CHEM	[illegible]	[illegible]
LARAGH, JOHN HENRY	CARDIOLOGY	[illegible]	[illegible]
LARDY, HENRY ARNOLD	BIOCHEMISTRY	1917	[illegible]
LARSEN, RUDOLF REINHOLD	PHYSICS	[illegible]	3111
LASSEN, NIELS ALEXANDER	PHYSIOLOGY	[illegible]	[illegible]
LAU, SUN HING	CARDIOLOGY	[illegible]	[illegible]
LAURELL, CARL-BERTIL	IMMUNOLOGY	[illegible]	[illegible]
LEBLOND, CHARLES PHILIPPE	HISTOLOGY	1910	[illegible]
LEDER, PHILIP	MOLEC BIOL	[illegible]	[illegible]
LEDERER, EDGAR	IMMUNOLOGY	[illegible]	[illegible]
LEE, BENJAMIN W	PHYSICS	[illegible]-77	[illegible]
LEE, TSUNG-DAO	PHYSICS	[illegible]	[illegible]
LEES, ROBERT SPENCER	CARDIOLOGY	[illegible]	[illegible]
LEFKOWITZ, ROBERT JOSEPH	PHARMACOLOGY	1943	[illegible]
LEHMANN, HERMANN	MOLEC BIOL	1910	[illegible]
LEHN, JEAN-MARIE	PHYSICAL CHEM	[illegible]	[illegible]
LEHNINGER, ALBERT LESTER	BIOCHEMISTRY	1917	[illegible]
LEONARD, NELSON JORDAN	BIOCHEMISTRY	1916	[illegible]
LE PICHON, XAVIER	GEOPHYSICS	1937	[illegible]
LEVER, ANTHONY F.	PHYSIOLOGY	[illegible]	[illegible]
LEVIN, WAYNE M.	PHARMACOLOGY	1940	[illegible]
• LEVINE, LAWRENCE	IMMUNOLOGY	[illegible]	[illegible]
LEVINE, RAPHAEL DAVID	PHYSICS	[illegible]	[illegible]
LEVY, GERHARD	PHARMACOLOGY	[illegible]	[illegible]
LEVY, ROBERT I.	CARDIOLOGY	1937	[illegible]
LEWIS, JACK	INORGANIC CHEM	[illegible]	5141
LI, CHOH HAO	PHARMACOLOGY	1913	4773
LICHTENSTEIN, LAWRENCE M.	IMMUNOLOGY	1934	[illegible]
• LIDDLE, GRANT WINDER	ENDOCRINOLOGY	1921	[illegible]
LIEBER, CHARLES SAUL	PATHOLOGY	1931	[illegible]
LINNANE, ANTHONY W.	BIOCHEMISTRY	[illegible]	[illegible]
LIPMANN, FRITZ A.	MICROBIOL	[illegible]	3173
• LIPSCOMB, WILLIAM NUNN	PHYSICAL CHEM	1919	[illegible]
LIPSETT, MORTIMER BROADWIN	ENDOCRINOLOGY	1921	[illegible]
LODISH, HARVEY FRANKLIN	CELL BIOL	1941	3517
LOENING, ULRICH EDUARD	MOLEC BIOL	1931	[illegible]
LONDON, WILLIAM THOMAS	ONCOLOGY	1932	[illegible]
LOWN, BERNARD	CARDIOLOGY	1921	[illegible]
LOWRY, OLIVER HOWE	BIOCHEMISTRY	1910	[illegible]
LU, ANTHONY Y.H.	PHARMACOLOGY	1937	[illegible]
LUDERITZ, OTTO	MICROBIOL	1920	[illegible]
LUFT, ROLF	ENDOCRINOLOGY	1914	[illegible]
LUTH, VERA	PHYSICS	[illegible]	[illegible]
• LYNCH, HARVEY LEE	PHYSICS	[illegible]	[illegible]
MACKANESS, GEORGE BELLAMY	IMMUNOLOGY	[illegible]	[illegible]
MACKAY, IAN REAY	IMMUNOLOGY	[illegible]	[illegible]
MACKLEM, PETER TIFFANY	PHYSIOLOGY	1931	[illegible]
MAHLER, HENRY RALPH	CELL BIOL	1921	[illegible]
• MAIZEL, JACOB V.	MOLEC BIOL	[illegible]	[illegible]
MALAISSE-LAGAE, FRANCINE	ENDOCRINOLOGY	[illegible]	[illegible]
MALAISSE, WILLY JEAN	ENDOCRINOLOGY	[illegible]	[illegible]
MANDEL, PAUL	CELL BIOL	[illegible]	[illegible]
MANN, JOSEPH BIRD	PHYSICS	[illegible]	[illegible]
MARCHALONIS, JOHN JACOB	IMMUNOLOGY	1940	[illegible]
MARCHESI, VINCENT T.	PATHOLOGY	[illegible]	[illegible]
MARCUS, RUDOLPH ARTHUR	PHYSICAL CHEM	[illegible]	[illegible]
• MAROKO, PETER RICHARD	CARDIOLOGY	[illegible]	[illegible]
MARTIN, GEORGE REILLY	PHARMACOLOGY	[illegible]	[illegible]
MASON, DEAN TOWLE	CARDIOLOGY	[illegible]	[illegible]
MASON, RONALD	INORGANIC CHEM	[illegible]	[illegible]
MASSEY, VINCENT	ENZYMOLOGY	[illegible]	[illegible]
MASSRY, SHAUL G.	PHYSIOLOGY	[illegible]	[illegible]
MATHE, GEORGES	CELL BIOL	[illegible]	[illegible]
• MAYER, JAMES WALTER	PHYSICS	[illegible]	[illegible]
• MCCULLOCH, ERNEST A.	CELL BIOL	[illegible]	[illegible]
MCCANN, SAMUEL MCDONALD	ENDOCRINOLOGY	[illegible]	[illegible]
• MCCARTHY, BRIAN JOHN	MICROBIOL	[illegible]	[illegible]
MCCONNELL, HARDEN MARSDEN	BIOPHYSICS	1927	[illegible]
MCDEVITT, HUGH O.	IMMUNOLOGY	[illegible]	[illegible]
MCELROY, MICHAEL BRENDAN	INORGANIC CHEM	[illegible]	[illegible]
MCGUIGAN, JAMES E.	GASTROENTEROL	1931	[illegible]
MCKUSICK, VICTOR A.	GENETICS	1921	[illegible]
MCLAFFERTY, FRED WARREN	ORGANIC CHEM	[illegible]	[illegible]
MEANS, ANTHONY R.	CELL BIOL	1941	[illegible]
MEISTER, ALTON	BIOCHEMISTRY	[illegible]	[illegible]
MEITES, JOSEPH	ENDOCRINOLOGY	1913	[illegible]
MELMON, KENNETH LLOYD	CELL BIOL	[illegible]	[illegible]
MELNICK, JOSEPH LOUIS	VIROLOGY	1914	[illegible]
• MERGENHAGEN, STEPHEN EDWARD	IMMUNOLOGY	[illegible]	[illegible]
MERIGAN, THOMAS CHARLES	VIROLOGY	[illegible]	[illegible]
MERRILL, JOHN PUTNAM	IMMUNOLOGY	1917	[illegible]
METCALF, DONALD	HEMATOLOGY	[illegible]	[illegible]
• MICHAEL, ALFRED FREDERICK	NEPHROLOGY	[illegible]	[illegible]
MILEY, MAX RAY	IMMUNOLOGY	[illegible]	[illegible]
• MIDGLEY, A. REES	ENDOCRINOLOGY	[illegible]	[illegible]
MIGEON, CLAUDE JEAN	ENDOCRINOLOGY	[illegible]	[illegible]
MILEDI, RICARDO	BIOPHYSICS	1927	[illegible]
MILIC-EMILI, JOSEPH	PHYSIOLOGY	1931	[illegible]
MILLER, EDWARD JOSEPH	BIOCHEMISTRY	[illegible]	[illegible]
MILLER, ELIZABETH CAVERT	ONCOLOGY	[illegible]	[illegible]
MILLER, JACQUES FRANCIS ALBERT	IMMUNOLOGY	1931	[illegible]
MILLER, JAMES ALEXANDER	ONCOLOGY	[illegible]	[illegible]
MILLER, WILLIAM HUGHES	PHYSICS	1941	[illegible]
MILSTEIN, CESAR	IMMUNOLOGY	1927	[illegible]
MIRSKY, ALFRED EZRA	CELL BIOL	[illegible]-74	[illegible]
MISLOW, KURT MARTIN	ORGANIC CHEM	[illegible]	[illegible]
MITCHELL, PETER DENNIS	BIOCHEMISTRY	[illegible]	[illegible]
MOLLER, GORAN	IMMUNOLOGY	[illegible]	[illegible]
MONCADA, S.	PHARMACOLOGY	1944	[illegible]
MONOD, JACQUES LUCIEN	MOLEC BIOL	1910-76	[illegible]
• MOORE, C. BRADLEY	PHYSICS	[illegible]	[illegible]
MOORE, GEORGE EUGENE	ONCOLOGY	[illegible]	[illegible]
MOREHOUSE, CHARLES CONOVER	PHYSICS	[illegible]	[illegible]
• MORINO, YONEZO	PHYSICAL CHEM	[illegible]	[illegible]
MORRE, D. JAMES	PLANT SCIENCES	[illegible]	[illegible]
MORRIS, HAROLD PAUL	ONCOLOGY	[illegible]	[illegible]
• MORROW, ANDREW GLENN	CARDIOLOGY	[illegible]	[illegible]
MORTON, DONALD CHARLES	ASTROPHYSICS	[illegible]	[illegible]
• MORTON, DONALD LEE	ONCOLOGY	[illegible]	[illegible]
MOTT, NEVILL FRANCIS	PHYSICS	[illegible]	[illegible]
• MUETTERTIES, EARL LEONARD	INORGANIC CHEM	1927	[illegible]
• MULLER-EBERHARD, HANS J.	IMMUNOLOGY	1927	[illegible]
MULLER, ACHIM	PHYSICS	[illegible]	[illegible]
MUNRO, HAMISH NISBET	BIOCHEMISTRY	1915	[illegible]
MURPHY, BEVERLY ELAINE P.	ENDOCRINOLOGY	[illegible]	[illegible]
MURPHY, DENNIS LUKE	NEUROPHARMACOL	[illegible]	[illegible]
• MUSTARD, JAMES FRASER	PATHOLOGY	1927	[illegible]
NAHMIAS, ANDRE JOSEPH	VIROLOGY	[illegible]	[illegible]
NAJARIAN, JOHN SARKIS	IMMUNOLOGY	1927	[illegible]
NAKANISHI, KOJI	ORGANIC CHEM	[illegible]	[illegible]
• NATHAN, DAVID GORDON	HEMATOLOGY	[illegible]	[illegible]
• NATHANS, DANIEL	MICROBIOL	[illegible]	[illegible]
• NATVIG, JACOB BIRGER	IMMUNOLOGY	1934	[illegible]
NEBERT, DANIEL WALTER	GENETICS	1940	[illegible]
NEFF, NORTON HERBERT	PHARMACOLOGY	[illegible]	[illegible]
NESS, NORMAN FREDERICK	PHYSICS	[illegible]	[illegible]

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NETA, PEDATSUR	ORGANIC CHEM	[illegible]	[illegible]
NEVILLE, DAVID MICHAEL	MOLEC BIOL	1934	3821
NICOLSON, GARTH LAMB	CELL BIOL	1943	6047
NILSSON, INGA MARIE	HEMATOLOGY	[illegible]	2814
• NIRENBERG, MARSHALL WARREN	GENETICS	1927	2814
NISHIMURA, SUSUMU	MOLEC BIOL	1931	[illegible]
NISWENDER, GORDON DEAN	ENDOCRINOLOGY	1940	3748
NOMURA, MASAYASU	MOLEC BIOL	1927	5174
NORMAN, ANTHONY WESTCOTT	BIOCHEMISTRY	[illegible]	3482
NORTHCOTE, DONALD HENRY	CELL BIOL	1921	2545
NOSSAL, GUSTAV J.V.	IMMUNOLOGY	1931	[illegible]
NOVIKOFF, ALEX BENJAMIN	PATHOLOGY	1913	2753
NOZAKI, HITOSI	ORGANIC CHEM	1922	[illegible]
NUSSENZWEIG, VICTOR	IMMUNOLOGY	1928	[illegible]
O'BRIEN, JOHN S.	GENETICS	1934	3731
• O'MALLEY, BERT W	ENDOCRINOLOGY	1936	6544
OCHOA, SEVERO	BIOCHEMISTRY	1905	2402
ODELL, WILLIAM DOUGLAS	PHYSIOLOGY	[illegible]	[illegible]
OHNO, SUSUMU	GENETICS	1928	2732
• OKE, JOHN BEVERLEY	ASTRONOMY	[illegible]	2437
OLAH, GEORGE ANDREW	ORGANIC CHEM	1927	7910
• OLD, LLOYD JOHN	ONCOLOGY	1933	[illegible]
OLSON, LARS O.	NEUROBIOLOGY	1942	[illegible]
OPPENHEIM, JOOST J.	IMMUNOLOGY	1934	[illegible]
ORCI, LELIO	HISTOLOGY	1937	[illegible]
ORLOFF, JACK	PHYSIOLOGY	1921	[illegible]
ORRENIUS, STEN G.	PHARMACOLOGY	1937	[illegible]
OSBORN, JOHN ANTHONY	INORGANIC CHEM	[illegible]	[illegible]
OSBORN, MARY	CELL BIOL	[illegible]	10876
OSKI, FRANK A.	HEMATOLOGY	[illegible]	[illegible]
OSTRIKER, JEREMIAH P.	ASTROPHYSICS	1937	[illegible]
OVCHINNIKOV, YU A.	BIOCHEMISTRY	1934	[illegible]
OWEN, CHARLES ARCHIBALD	HEMATOLOGY	1915	[illegible]
OWMAN, CHRISTER SVEN OLLE	HISTOLOGY	[illegible]	[illegible]
PACKER, LESTER	BIOPHYSICS	[illegible]	[illegible]
PACKHAM, MARIAN AITCHISON	HEMATOLOGY	1927	[illegible]
PAGE, IRVINE HEINLY	PHYSIOLOGY	1901	2716
PALADE, GEORGE E.	CELL BIOL	1912	[illegible]
PALKOVITS, MIKLOS	NEUROPHARMACOL	[illegible]	[illegible]
PAPAHADJOPOULOS, DEMETRIOS P.	MOLEC BIOL	1934	[illegible]
PAQUETTE, LEO ARMAND	ORGANIC CHEM	1934	5116
PARDEE, ARTHUR BECK	CELL BIOL	1921	3110
PARK, CHARLES RAWLINSON	PHYSIOLOGY	1916	[illegible]
PARKER, CHARLES WARD	IMMUNOLOGY	[illegible]	[illegible]
PARKS, W.P.	IMMUNOLOGY	1941	[illegible]
PARMLEY, WILLIAM W	CARDIOLOGY	[illegible]	[illegible]
PASTAN, IRA HARRY	BIOCHEMISTRY	1931	[illegible]
PATERSON, JAMES MCEWAN	PHYSICS	1937	3117
PAUL, JOHN	MOLEC BIOL	[illegible]	[illegible]
PAUL, WILLIAM ERWIN	IMMUNOLOGY	[illegible]	[illegible]
PEARSE, ANTHONY GUY EVERSON	ENDOCRINOLOGY	1916	[illegible]
PEARSON, RALPH GOTTFRID	INORGANIC CHEM	1919	[illegible]
• PENMAN, SHELDON	MOLEC BIOL	[illegible]	[illegible]
• PERL, MARTIN LOUIS	PHYSICS	1927	[illegible]
PERLMANN, PETER	IMMUNOLOGY	1919	[illegible]
PERRY, ROBERT PALESE	MOLEC BIOL	1931	[illegible]
PERUTZ, MAX FERDINAND	MOLEC BIOL	1914	[illegible]
PETERLIN, ANTON	PHYSICS	[illegible]	[illegible]
PHILIPSON, LENNART	MICROBIOL	[illegible]	[illegible]
PHILLIPS, DAVID CHILTON	MOLEC BIOL	[illegible]	[illegible]
PHILLIPS, JAMES CHARLES	PHYSICS	[illegible]	[illegible]
PHILLIPS, R.J.	PHYSICS	[illegible]	[illegible]
PHILLIPS, ROBERT ALLAN	IMMUNOLOGY	1937	[illegible]
PIEZ, KARL ANTON	BIOCHEMISTRY	[illegible]	[illegible]
PITOT, HENRY C.	ONCOLOGY	[illegible]	[illegible]
PITT, BERTRAM	CARDIOLOGY	[illegible]	[illegible]
PLETSCHER, ALFRED	NEUROPHARMACOL	1917	[illegible]
POLAK, JULIA MARGARET	HISTOLOGY	[illegible]	[illegible]
POLANYI, JOHN CHARLES	PHYSICAL CHEM	1929	[illegible]
• POPLE, JOHN ANTHONY	THEORETICAL CHEM	1925	12714
POPPER, HANS	PATHOLOGY	[illegible]	[illegible]
PORATH, JERKER OLOF	BIOCHEMISTRY	1921	[illegible]
PORTE, DANIEL	ENDOCRINOLOGY	1931	[illegible]
PORTER, JOHN CHARLES	PHYSIOLOGY	[illegible]	[illegible]
• PORTER, JOHN WILLARD	BIOCHEMISTRY	1915	[illegible]
• PORTER, KEITH ROBERTS	MOLEC BIOL	1912	[illegible]
PORTER, KENDRICK ARTHUR	PATHOLOGY	[illegible]	2781
• POSTE, GEORGE HENRY	PATHOLOGY	1944	[illegible]
• POTTER, MICHAEL	ONCOLOGY	1924	[illegible]
POTTER, VAN RENSSELAER	CELL BIOL	1911	[illegible]
POTTS, JOHN THOMAS	ENDOCRINOLOGY	[illegible]	[illegible]
POWELL, THOMAS PHILIP STROUD	NEUROLOGY	[illegible]	[illegible]
PRESSMAN, BERTON CHARLES	PHARMACOLOGY	[illegible]	[illegible]
• PRESSMAN, DAVID	IMMUNOLOGY	1916	[illegible]
• PREUSSMANN, R.	ONCOLOGY	[illegible]	[illegible]
PRINCE, ALFRED M.	VIROLOGY	[illegible]	[illegible]
• PROCKOP, DARWIN J.	BIOCHEMISTRY	[illegible]	[illegible]
PURCELL, ROBERT HARRY	VIROLOGY	[illegible]	[illegible]
QUIE, PAUL GERHARDT	IMMUNOLOGY	[illegible]	[illegible]
RABINOWITZ, MURRAY	MOLEC BIOL	1927	[illegible]
• RACKER, EFRAIM	BIOCHEMISTRY	1913	[illegible]
RAFF, MARTIN C.	CELL BIOL	[illegible]	[illegible]
• RAISZ, LAWRENCE GIDEON	ENDOCRINOLOGY	[illegible]	[illegible]
RAMIREZ, FAUSTO	ORGANIC CHEM	[illegible]	[illegible]
RAPP, FRED	VIROLOGY	[illegible]	[illegible]
RAPP, HERBERT JOSEPH	IMMUNOLOGY	[illegible]	[illegible]
RASMUSSEN, HOWARD	CELL BIOL	[illegible]	[illegible]
RATNOFF, OSCAR DAVIS	HEMATOLOGY	1916	[illegible]
• RAWLS, WILLIAM EDGAR	VIROLOGY	[illegible]	[illegible]
RAZIN, SHMUEL	MICROBIOL	[illegible]	[illegible]
RECTOR, FLOYD CLINTON	PHYSIOLOGY	[illegible]	[illegible]
REDDING, TOMMIE W	ENDOCRINOLOGY	[illegible]	[illegible]
REEDER, DON DAVID	PHYSICS	[illegible]	[illegible]
REESE, THOMAS SARGENT	CELL BIOL	[illegible]	[illegible]
• REICH, EDWARD	BIOCHEMISTRY	1927	[illegible]
• REICHERT, LEO E.	ENDOCRINOLOGY	[illegible]	[illegible]
REISFELD, RALPH ALFRED	IMMUNOLOGY	[illegible]	[illegible]
• REMEIKA, J.P.	PHYSICS	[illegible]	[illegible]
REMINGTON, JACK SAMUEL	MICROBIOL	1931	[illegible]
• REMMER, HERBERT	PHARMACOLOGY	1919	[illegible]
RENOLD, ALBERT ERNST	ENDOCRINOLOGY	[illegible]	[illegible]
• REYNOLDS, JACQUELINE ANN	BIOPHYSICS	[illegible]	[illegible]
RICE, STUART ALLEN	PHYSICAL CHEM	1932	[illegible]
RICH, ALEXANDER	MOLEC BIOL	1924	4611
RICHARDSON, CHARLES CLIFTON	MOLEC BIOL	[illegible]	[illegible]
RICHTER, BURTON	PHYSICS	1931	3731
RIVIER, JEAN E.F.	ENDOCRINOLOGY	1941	[illegible]
ROBBINS, JOHN BENNETT	MICROBIOL	[illegible]	[illegible]
ROBERTS, JOHN D	ORGANIC CHEM	1918	[illegible]
ROBERTS, WILLIAM CLIFFORD	CARDIOLOGY	1932	[illegible]
ROBERTSON, J.I.S.	CARDIOLOGY	[illegible]	[illegible]
ROBINS, ROLAND KENITH	ORGANIC CHEM	[illegible]	[illegible]
ROBISON, GEORGE ALAN	PHARMACOLOGY	1934	[illegible]
RODBELL, MARTIN	ENDOCRINOLOGY	1925	[illegible]
ROEDER, ROBERT G.	BIOCHEMISTRY	1942	[illegible]
• ROITT, IVAN MAURICE	IMMUNOLOGY	1927	[illegible]
ROIZMAN, BERNARD	VIROLOGY	[illegible]	[illegible]
ROSEMAN, SAUL	BIOCHEMISTRY	[illegible]	[illegible]
• ROSEN, FRED SAUL	IMMUNOLOGY	[illegible]	[illegible]
ROSEN, KENNETH M.	CARDIOLOGY	1937	[illegible]
ROSENBERG, SAUL A.	ONCOLOGY	1927	[illegible]
ROSENTHAL, ALAN S	IMMUNOLOGY	[illegible]	[illegible]
ROSS, GRIFF TERRY	ENDOCRINOLOGY	[illegible]	[illegible]
ROSS, JOHN	CARDIOLOGY	[illegible]	[illegible]
• ROSS, RUSSELL	CELL BIOL	[illegible]	[illegible]
• ROTH, JESSE	ENDOCRINOLOGY	1934	[illegible]
ROTH, ROBERT HENRY	PHARMACOLOGY	[illegible]	[illegible]
• ROWE, DAVID STUART	IMMUNOLOGY	[illegible]	[illegible]
ROWE, WALLACE PRESCOTT	VIROLOGY	[illegible]	[illegible]
• RUBBIA, CARLO	PHYSICS	1934	[illegible]
RUBIN, EMANUEL	PATHOLOGY	[illegible]	2781
RUBIN, HARRY	CELL BIOL	[illegible]	[illegible]
RUDDLE, FRANCIS HUGH	GENETICS	[illegible]	[illegible]
RUDOLPH, ABRAHAM MORRIS	PHYSIOLOGY	1924	[illegible]
• RUOSLAHTI, ERKKI ILMARI	ONCOLOGY	1940	[illegible]
RUSSELL, DIANE HADDOCK	PHARMACOLOGY	[illegible]	[illegible]
RUTTER, WILLIAM J.	MOLEC BIOL	[illegible]	[illegible]
RYAN, KENNETH JOHN	ENDOCRINOLOGY	[illegible]	[illegible]
SACHS, LEO	ONCOLOGY	1924	[illegible]
SAH, CHIH-TANG	PHYSICS	1932	[illegible]
SALPETER, EDWIN ERNEST	ASTRONOMY	1924	[illegible]
• SALTIN, BENGT	PHYSIOLOGY	[illegible]	2777
• SAMBROOK, JOE	VIROLOGY		[illegible]
SAMUELSSON, BENGT	BIOCHEMISTRY	1934	[illegible]
SANDAGE, ALLAN REX	ASTRONOMY	[illegible]	[illegible]
SANDBERG, AVERY ASA	CELL BIOL	1921	[illegible]
SANDLER, MERTON	PHARMACOLOGY	[illegible]	[illegible]
SANGER, FREDERICK	MOLEC BIOL	1918	[illegible]
SATCHLER, GEORGE RAYMOND	PHYSICS	[illegible]	[illegible]
SATO, RYO	BIOCHEMISTRY	[illegible]	[illegible]
SAWYER, CHARLES HENRY	ENDOCRINOLOGY	1915	[illegible]
SCHAEFER, HENRY FREDERICK	THEORETICAL CHEM	1944	[illegible]
SCHAFFNER, FENTON	PATHOLOGY	[illegible]	[illegible]
SCHALCH, DON SYLVESTER	ENDOCRINOLOGY	[illegible]	[illegible]
SCHALLY, ANDREW VICTOR	ENDOCRINOLOGY	1926	[illegible]
SCHERAGA, HAROLD ABRAHAM	PHYSICAL CHEM	1921	[illegible]
SCHERLAG, BENJAMIN J.	CARDIOLOGY	[illegible]	[illegible]
SCHILDKRAUT, JOSEPH JACOB	PSYCHIATRY	1934	[illegible]
SCHIMKE, ROBERT T.	MOLEC BIOL	1932	[illegible]
SCHLESINGER, DAVID	MOLEC BIOL	[illegible]	[illegible]
SCHLEYER, PAUL VON RAGUE	PHYSICAL CHEM	1930	[illegible]
SCHMELTEKOPF, ARTHUR L.	PHYSICS	[illegible]	[illegible]
• SCHMID, HANS	PHYSICAL CHEM	1917-78	[illegible]
SCHMIDBAUR, HUBERT	ORGANOMET CHEM	1934	[illegible]
SCHRAUZER, GERHARD N.	INORGANIC CHEM	1932	[illegible]
SCHULTZ, STANLEY GEORGE	PHYSIOLOGY	1931	[illegible]
SCHUR, PETER H.	IMMUNOLOGY	[illegible]	[illegible]
SCHWARTZ, ARNOLD	PHARMACOLOGY	[illegible]	[illegible]
SCHWARTZ, ROBERT STEWART	IMMUNOLOGY	[illegible]	[illegible]
• SCHWITTERS, ROY FREDERICK	PHYSICS	1944	[illegible]
SCOLNICK, EDWARD M.	VIROLOGY	1940	[illegible]
SEEGMILLER, JARVIS EDWIN	IMMUNOLOGY	[illegible]	[illegible]
SEEMAN, PHILIP	NEUROLOGY	1934	[illegible]
SEGAL, GERALD A.	PHYSICS	[illegible]	[illegible]
SELA, MICHAEL	IMMUNOLOGY	1924	[illegible]
SELDIN, DONALD WAYNE	PHYSIOLOGY	[illegible]	[illegible]
SELIGMANN, MAXIME G.	HEMATOLOGY	1927	[illegible]
SELL, STEWART	IMMUNOLOGY	[illegible]	[illegible]
SETLOW, RICHARD BURTON	BIOPHYSICS	1921	[illegible]
SEVER, JOHN LOUIS	MICROBIOL	1932	[illegible]
SEYFERTH, DIETMAR	ORGANOMET CHEM	[illegible]	[illegible]
• SHAPIRO, ARNOLD L.	BIOPHYSICS	[illegible]	[illegible]
SHARON, NATHAN	BIOCHEMISTRY	[illegible]	[illegible]
• SHARP, PHILIP ALLEN	MOLEC BIOL	1944	[illegible]
SHATKIN, AARON JEFFREY	VIROLOGY	[illegible]	[illegible]
SHAW, BERNARD LESLIE	INORGANIC CHEM	1930	[illegible]
SHERLOCK, SHEILA	GASTROENTEROL	1918	[illegible]
SHEVACH, ETHAN MENAHEM	IMMUNOLOGY	[illegible]	[illegible]
• SHIRANE, GEN	PHYSICS	[illegible]	[illegible]
SHIRLEY, DAVID ARTHUR	PHYSICAL CHEM	[illegible]	[illegible]
SHREFFLER, DONALD CECIL	GENETICS	[illegible]	[illegible]
• SHULMAN, NAHUM RAPHAEL	HEMATOLOGY	[illegible]	[illegible]
• SHULMAN, ROBERT GERSON	CHEM PHYS	[illegible]	[illegible]
SIEKEVITZ, PHILIP	CELL BIOL	1918	3424
SIESJO, BO K.	NEUROLOGY	[illegible]	[illegible]
SIMINOVITCH, LOUIS	GENETICS	[illegible]	[illegible]
SIMMONS, RICHARD LAWRENCE	IMMUNOLOGY	1934	[illegible]
• SIMPSON, WILLIAM TRACEY	THEORETICAL CHEM	[illegible]	[illegible]
• SIMS, PETER	MOLEC BIOL	[illegible]	[illegible]
SINGER, SEYMOUR JONATHAN	CELL BIOL	1924	[illegible]
SINSHEIMER, ROBERT LOUIS	MOLEC BIOL	[illegible]	[illegible]
SJOERDSMA, ALBERT	PHARMACOLOGY	1924	[illegible]
• SJOGREN, HANS OLOV	ONCOLOGY	[illegible]	[illegible]
SJOQVIST, FOLKE F.G.	PHARMACOLOGY	[illegible]	[illegible]

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SJOVALL, JAN	BIOCHEMISTRY	1926	2814
* SKOOG, FOLKE	PLANT SCIENCES	1908	2501
* SMALL, DONALD MACFARLAND	BIOPHYSICS	1931	2522
SMITH, EMIL L.	BIOCHEMISTRY	1911	2612
* SMITH, IAN CORMACK PALMER	BIOPHYSICS	1939	2876
✓ SMITH, THOMAS WOODWARD	CARDIOLOGY	1936	4027
* SNYDER, FRED	BIOCHEMISTRY	1931	3172
SNYDER, SOLOMON HALBERT	PHARMACOLOGY	1938	13148
✓ SOBEL, BURTON E	CARDIOLOGY	1937	4634
SOELDNER, JOHN STUART	ENDOCRINOLOGY	1932	2700
SONNENBLICK, EDMUND H	CARDIOLOGY	1932	6201
* SORM, FRANTISEK	ORGANIC CHEM	1913	4880
SPECTOR, SYDNEY	PHARMACOLOGY	1923	2702
SPELLACY, WILLIAM NELSON	ENDOCRINOLOGY	1934	2676
SPICER, SAMUEL SHERMAN	PATHOLOGY	1914	3131
SPICER, WILLIAM EDWARD	PHYSICS	1929	3480
SPIEGELMAN, SOLOMON	MOLEC BIOL	1914	8415
SPIRO, ROBERT GUNTER	BIOCHEMISTRY	1929	3036
* SPRENT, JONATHAN	IMMUNOLOGY	1941	3045
SPURR, ARTHUR RICHARD	PLANT SCIENCES	1915	2716
STADTMAN, EARL REECE	BIOCHEMISTRY	1919	2626
* STANIER, ROGER YATE	MICROBIOL	1916	2608
STARZL, THOMAS E.	IMMUNOLOGY	1926	5119
✓ STECK, THEODORE LYLE	BIOCHEMISTRY	1939	4457
STEIN, YECHEZKIEL	CELL BIOL	1926	2496
STEINBERG, ALFRED DAVID	IMMUNOLOGY	1940	2673
STEINBERG, DANIEL	BIOCHEMISTRY	1922	3025
STEINER, ALTON L.	CELL BIOL	1936	2865
STEINER, DONALD FREDERICK	ENDOCRINOLOGY	1930	3421
* STEPHENS, FRANK SAMUEL	THEORETICAL CHEM	1931	2789
* STEPHENSON, J.R.	VIROLOGY	1942	2960
* STEWART, ROBERT F.	PHYSICS	1936	5611
STOECKENIUS, WALTHER	BIOPHYSICS	1921	2471
✓ * STONE, FRANCIS GORDON ALBERT	ORGANOMET CHEM	1925	4013
STORB, RAINER F.	IMMUNOLOGY	1935	3960
* STROBER, WARREN	IMMUNOLOGY	1937	2621
* STROMINGER, JACK L.	VIROLOGY	1925	6012
✓ STUDIER, FREDERICK WILLIAM	MOLEC BIOL	1936	4050
SUGIMURA, TAKASHI	ONCOLOGY	1926	2637
SUMMERSKILL, WILLIAM HEDLEY	GASTROENTEROL	1926-77	2631
✓ SUNDARALINGAM, MUTTAIYA	BIOPHYSICS	1931	4022
SUTHERLAND, EARL WILBUR	PHARMACOLOGY	1915-74	10897
SUTNICK, ALTON IVAN	ONCOLOGY	1928	2706
SUZUKI, KUNIHIKO	NEUROLOGY	1932	2542
SVEJGAARD, ARNE	IMMUNOLOGY	1937	2436
✓ * SWAN, HAROLD JAMES	PHYSIOLOGY	1922	4136
SWEELEY, CHARLES CRAWFORD	ORGANIC CHEM	1930	2651
✓ SYMONS, MARTYN CHRISTIAN R.	INORGANIC CHEM	1925	4580
* SZWARC, MICHAEL	PHYSICAL CHEM	1909	2960
SZYBALSKI, WACLAW	MOLEC BIOL	1921	2860
✓ * TAKEUCHI, TOMIO	MICROBIOL	1923	4460
TALAL, NORMAN	IMMUNOLOGY	1934	3050
TAN, ENG M.	IMMUNOLOGY	1936	2703
* TANFORD, CHARLES	BIOCHEMISTRY	1921	5404
TAPPEL, ALOYS LOUIS	BIOCHEMISTRY	1926	4208
✓ TASHJIAN, ARMEN HAIG	ENDOCRINOLOGY	1932	3011
* TATA, JAMSHED RUSTOM	BIOCHEMISTRY	1930	2867
TAUBE, HENRY	INORGANIC CHEM	1915	2962
TAYLOR, EDWIN WILLIAM	BIOPHYSICS	1929	3461
TEMIN, HOWARD MARTIN	ONCOLOGY	1934	8570
* TERASAKI, PAUL ICHIRO	IMMUNOLOGY	1929	7379
TERRY, WILLIAM DAVID	IMMUNOLOGY	1933	2617
✓ THOENEN, HANS FRIEDRICH E.	NEUROLOGY	1928	4986
THOMAS, CHARLES ALLEN	GENETICS	1927	2707
✓ THOMAS, EDWARD DONNALL	ONCOLOGY	1920	4762
TILL, JAMES EDGAR	BIOPHYSICS	1931	2466
* TODARO, GEORGE JOSEPH	ONCOLOGY	1937	6600
TOLBERT, NATHAN EDWARD	PLANT SCIENCES	1919	2731
TOMASI, THOMAS B.	IMMUNOLOGY	1927	2915
TOMKINS, GORDON M.	BIOCHEMISTRY	1926-75	7252
TRILLING, GEORGE HENRY	PHYSICS	1930	3773
TROST, BARRY M.	ORGANIC CHEM	1941	3987
TRUMP, BENJAMIN FRANKLIN	PATHOLOGY	1932	3581
TSO, PAUL ON PONG	BIOPHYSICS	1929	3477
* TURK, JOHN LESLIE	PATHOLOGY	1930	3243
TURKINGTON, ROGER W.	ENDOCRINOLOGY	1936	2716
* TURRO, NICHOLAS JOHN	ORGANIC CHEM	1938	3101
UDENFRIEND, SIDNEY	BIOCHEMISTRY	1918	8641
UHR, JONATHAN WILLIAM	IMMUNOLOGY	1927	3805
* UMEZAWA, HAMAO	MICROBIOL	1914	7608
✓ UNANUE, EMIL R.	IMMUNOLOGY	1934	5100
UNGER, ROGER HAROLD	ENDOCRINOLOGY	1924	6030
✓ UNGERSTEDT, URBAN	PHARMACOLOGY	1942	4852
URRY, DAN WESLEY	BIOPHYSICS	1935	3096
* UTIGER, ROBERT D.	ENDOCRINOLOGY	1931	3394
VAGELOS, PINDAROS ROY	BIOCHEMISTRY	1929	2933
✓ VALE, WYLIE WALKER	NEUROENDOCRINOL	1941	4120
VALLEE, BERT L.	BIOCHEMISTRY	1919	4829
VAN DEENEN, LAURENS L.M.	BIOCHEMISTRY	1928	8267
VAN ROOD, JOHANNES JOSEPH	IMMUNOLOGY	1926	2592
* VAN UITERT, L.G.	PHYSICS	1922	2460
VANE, JOHN ROBERT	PHARMACOLOGY	1927	9671
VANNUCCI, FRANCOIS M	PHYSICS	1944	2908
VAUGHAN, MARTHA	BIOCHEMISTRY	1926	3572
VENABLE, JOHN HOWARD	CELL BIOL	1929	3241
VENEZIANO, G.	PHYSICS	1942	2965
VESELL, ELLIOT S.	PHARMACOLOGY	1933	2504
VINOGRAD, JEROME	MOLEC BIOL	1913-76	4185
VINUELA, ELADIO	VIROLOGY	1937	3471
* VOGT, PETER KLAUS	VIROLOGY	1932	3728
WABER, JAMES T.	PHYSICS	1920	2948
WAGNER, HENRY NICHOLAS	NUCLEAR MED	1927	4454
WAKSMAN, BYRON HALSTEAD	IMMUNOLOGY	1919	3405
WALDMANN, THOMAS A.	IMMUNOLOGY	1930	4054
* WALLACH, DONALD F.H.	ONCOLOGY	1926	5423
WALSH, DONALD ARTHUR	BIOCHEMISTRY	1939	2400
* WALSH, JOHN HARLEY	GASTROENTEROL	1938	2987
WALTER, RODERICH	PHYSIOLOGY	1937-79	3857
✓ WARD, PETER A.	PATHOLOGY	1934	4044
WARNER, NOEL L.	IMMUNOLOGY	1935	3436
WARREN, KENNETH S.	IMMUNOLOGY	1929	2447
WEATHERALL, DAVID JOHN	HEMATOLOGY	1933	3232
WEBER, KLAUS	BIOCHEMISTRY	1936	13427
WEIBEL, EWALD RUDOLF	PHYSIOLOGY	1929	2958
* WEIGLE, WILLIAM O.	IMMUNOLOGY	1927	2723
WEINBERG, STEVEN	PHYSICS	1933	7867
WEINSTEIN, LOUIS	MICROBIOL	1909	3601
WEISBURGER, JOHN HANS	PHARMACOLOGY	1921	3457
WEISS, BENJAMIN	PHARMACOLOGY	1937	2888
WEISS, HARVEY JEROME	HEMATOLOGY	1929	3260
WEISSBACH, HERBERT	MOLEC BIOL	1932	3385
WEISSMANN, GERALD	CELL BIOL	1930	5072
WENKERT, ERNEST	ORGANIC CHEM	1925	2637
* WESSELLS, NORMAN KEITH	CELL BIOL	1932	2791
WEST, THOMAS SUMMERS	THEORETICAL CHEM	1927	3214
* WESTPHAL, OTTO HERMANN E.	BIOCHEMISTRY	1913	3139
WHITAKER, J.S.	PHYSICS	1948	2674
✓ * WHITE, JAMES G.	PATHOLOGY	1929	4982
WHITESIDES, GEORGE M.	ORGANIC CHEM	1939	2625
* WIBERG, KENNETH BERLE	ORGANIC CHEM	1927	2798
WIDE, LEIF EDVIN	ENDOCRINOLOGY	1934	2687
WIESEL, TORSTEN N.	PHYSIOLOGY	1924	3934
WIGZELL, HANS L.R.	IMMUNOLOGY	1938	7243
✓ * WILKINSON, GEOFFREY	ORGANOMET CHEM	1921	5444
✓ WILLIAMS, DUDLEY HOWARD	ORGANIC CHEM	1937	5501
WILLIAMS, R. TECWYN	PHARMACOLOGY	1909-79	2790
✓ WILLIAMS, RALPH C.	IMMUNOLOGY	1928	4037
WILLIAMS, ROBERT HARDIN	ENDOCRINOLOGY	1909-79	3290
WILLIAMS, ROBERT JOSEPH P.	INORGANIC CHEM	1926	2942
✓ WILLIAMS, ROGER	GASTROENTEROL	1933	5204
WILLIAMSON, JOHN RICHARD	BIOCHEMISTRY	1933	2888
* WILSON, DAVID F.	BIOCHEMISTRY	1938	2713
WILSON, JEAN DONALD	ENDOCRINOLOGY	1932	3290
* WILSON, KENNETH GEDDES	PHYSICS	1936	3081
WINEFORDNER, JAMES D.	PHYSICS	1931	2811
WINSTEIN, SAUL	PHYSICAL CHEM	1912-69	3146
* WITKOP, BERNHARD	ORGANIC CHEM	1917	3621
WITTMANN, HEINZ GUENTER	MOLEC BIOL	1927	2776
WOLFF, JAN	ENDOCRINOLOGY	1925	2584
WOLFF, SHELDON M.	IMMUNOLOGY	1930	2981
WOODWARD, ROBERT BURNS	ORGANIC CHEM	1917-79	3844
WURTMAN, RICHARD JAY	ENDOCRINOLOGY	1936	6580
WYMAN, JEFFRIES	MOLEC BIOL	1901	4120
* YAGI, HARUHIKO	ORGANIC CHEM	1939	2841
YALOW, ROSALYN SUSSMAN	ENDOCRINOLOGY	1921	4900
YANOFSKY, CHARLES	MOLEC BIOL	1925	4954
✓ * YEN, SAMUEL SHOW-CHIH	ENDOCRINOLOGY	1927	4108
YOUNG, ROBERT C	ONCOLOGY	1940	2503
YUNIS, EDMOND J.	IMMUNOLOGY	1929	3611
* ZBAR, BERTON	ONCOLOGY	1930	3131
ZECH, LORE	GENETICS	1923	3363
ZIFF, MORRIS	IMMUNOLOGY	1913	2742
ZIMMERMAN, HOWARD ELLIOT	ORGANIC CHEM	1926	3226
ZINDER, NORTON DAVID	MOLEC BIOL	1928	2529
* ZUMINO, BRUNO	PHYSICS	1923	2560

Keep in mind, however, that we have eliminated from this study not only all citations to work published before 1965, but also citations to any articles not included in our source files. Books and book chapters were not added to the source file until 1977 and they have been excluded from the study. Citations to books are therefore also excluded. Since we cover all of the most important journals of science it is not a great limitation. But there is always a chance that some highly cited individual may be adversely affected by these selection criteria.

Of course, many scientists who published their most-cited papers before the starting date of this study, 1965, do not appear on this list. So there is a chronological bias in this study favoring those

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who have published mainly during this period. As we change the chronological scope of our studies the list will change. In fact, if you compare the 300 most-cited authors on this list to those in the earlier study,[2] you would find that only one third of them are new names which did not appear on the 1961-1976 list. The new names are indicated by a check mark in Table 1. As a rough estimate, about 7.5 percent of the names will change as the base year is shifted annually.

I would like to mention a few of the mechanics involved in preparing these lists to show what care is taken to avoid errors of one kind or another. Once we had matched the citations with the source entries, the computer was programmed to generate a ranked list of authors' "names" (surname and up to three initials), a bibliography of papers associated with those names, and the number of citations each of those articles received. Actually, about 85 percent of the names could be used immediately but, unfortunately, about 15 percent represented homographs. Many journals do not provide the authors' first names in by-lines. And practically all journals only include initials for first names in reference citations. So we could not easily include first names in the *Citation Index* even if we wished to use the extra space.

When you look up a name like R.A. Fisher in *SCI*, the boldface entry is really a heading for the homograph which includes several people with the same last name and initials. It is never a problem to locate a particular paper, and ordinarily it is not a problem to differentiate the papers of the geneticist from the physicist. But in this study we religiously checked out each homograph to determine which papers belonged to each of the two or more persons involved.

We went about making this determination in a number of ways. One of the methods we used involved checking ISI's *Current Bibliographic Directory of the Arts & Sciences®* for the authors involved. Although we could confidently eliminate some of the addresses by comparing journal titles with department names, we sent letters to the authors at the remaining addresses. The letter asked them to fill out a questionnaire and provide a complete list of their publications. A large percentage of authors responded to this initial contact. For those who did not, or for those for whom we had obsolete addresses, we sent out second letters or attempted to contact them by telephone. In this way we were able to establish complete first and middle name information and discover which articles belonged to which authors.

As a result of these efforts, we eliminated many names and resolved the homograph problem. Thus, we counted only the number of citations and papers properly attributable to most authors. However, some authors may find that they are credited with a few extra papers or citations. In these cases, the few papers and citations provided insignificant "noise."

The average number of citations for all authors listed here was 3,811—over 272 citations per year. As it turns out, the minimum threshold for inclusion in this study was 2,436 citations—an average of about 174 citations per year. The average *cited* author is cited less than one tenth as often.

The average author in this study published 121 papers; on 32 of these his or her name appeared as first author and on 89 as one of the subsequent authors. The average author received 1,178 citations to papers on which he or she appeared as first author and 2,633 as coauthor. This clearly demonstrates the importance of all-author data for the most-cited scientists.

The list is provided in alphabetical, rather than ranked, order. For each author we have identified the field. To avoid further delay in publishing the list

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we decided to provide this initial look now. In future parts of this study we will discuss groups of authors by disciplines. A cautionary note is needed here. Citation practices vary among disciplines so the reader should be careful in comparing raw citation counts for authors in one field with counts for authors in another. More detailed information about each author will be given in the latter parts of this study.

You will notice that for the first time in one of these studies we are giving the complete first and middle names of authors to better identify them. Based on purely feminine given names, it seems that at least 23 women appear on this list. We are not sure of the exact number because we did not ask the authors to state their gender on the questionnaire. In the future I plan an essay about women in science. The women identified in this study will be a good starting point.

We have also listed the date of birth for each author and, where applicable, date of death. The oldest author on this list is F.A. Lipmann, born in 1899, and the youngest is J.S. Whitaker, born in 1948. The average age of the authors on the list is 53. Table 2 shows the number of authors by decade of birth. Now it is clear that it would have been of interest to use the age at which their first paper was published, but we did not ask for this information.

Table 2: Number of authors by decade of birth.

Decade	Number of authors
1899	1
1900-1909	25
1910-1919	122
1920-1929	390
1930-1939	381
1940-1948	80

(One birth date was unavailable.)

The questionnaires sent to the authors asked them to name their own field or discipline. The authors are thus self-categorized. Thirty-eight fields are represented on the list. Table 3 lists the number of authors in each field. Immunology is the largest group.

An asterisk next to an author's name indicates that some of our information about that author may be incomplete. Such authors may never have sent us a bibliography or didn't fill out the questionnaire completely so we were unable to verify a piece of information. The information presented is accurate to the best of our knowledge. But errors may occur in such large studies.

With little effort, readers can scan the list to learn who is the most-cited author. In the vast majority of cases appearance on this list is an *indicator* that the person involved is *of Nobel class*.[4] Harriet Zuckerman, in her book *Scien-*

Table 3: Number of authors per field.

Field	Number of authors
Aeronomy	2
Astronomy	5
Astrophysics	7
Biochemistry	84
Biophysics	26
Cardiology	33
Cell Biology	57
Chemical Physics	2
Endocrinology	74
Enzymology	6
Gastroenterology	10
Genetics	21
Geophysics	2
Hematology	21
Histology	8
Immunology	128
Inorganic Chemistry	27
Microbiology	18
Molecular Biology	67
Nephrology	1
Neurobiology	2
Neuroendocrinology	1
Neurology	13
Neuropharmacology	9
Nuclear Medicine	1
Oncology	48
Organic Chemistry	51
Organometallic Chemistry	10
Pathology	23
Pharmacology	59
Physical Chemistry	24
Physics	77
Physiology	36
Plant Sciences	6
Psychiatry	4
Surgery	1
Theoretical Chemistry	7
Virology	29

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tific Elite, likens this group to the "immortals," who, though equal in stature, are not included in the French Academy's limited membership of 40. She refers to these individuals "...who are peers of prizewinners in every sense except that of having the award" as occupants of the "forty-first chair."[5] (p. 42) Such people can be identified as *of Nobel class*. In fact, 41 Nobelists do appear on the list. However, one would have to examine the data for each individual on the list, in combination with other factors, to determine whether appearance on this list is primarily due to productivity, citation impact, or both. One cannot conclude that the most-cited or the most published author is necessarily the most important or the one who has made the greatest contribution to science.

Any list is, of course, only as good as the system that helps create it. Many of you are well aware of quirks in authorship attribution that permit some scientist-administrators to put their names on hundreds of papers as coauthors. Some of these scientist-administrators will show up on the accompanying list. This follows the old European tradition and quite often it has been followed at certain American institutions. The list may also include certain academicians who have not otherwise achieved great distinction but have coauthored dozens of papers with graduate students. We should not be quick to criticize such authors since there is no agreed-upon, published set of rules for establishing authorship. In some instances the other authors may have been only too happy to have the prestigious name included. It often helps the young research worker to be joined by a famous scientist as coauthor. There is as yet no agreed-upon method for assigning credit to each coauthor, although Derek Price has recently suggested a method to fairly attribute citations on multi-authored papers.[6] When we do the institutional study of these authors, this particular problem will be eliminated since citations to each paper can be credited to the department involved.

We realized long ago that a few people may appear on such lists because they had written one super-cited paper. When I discussed this with John deCani, department of statistics, University of Pennsylvania, he suggested that we might "censor" the data by excluding citations to the most-cited paper for each of the 1,000 authors.[7]

We did so and found that 90 percent of the time the most-cited paper accounted for fewer than 22 percent of the citations. In other words, the vast majority of the authors on this list would still remain among the most-cited if their most-cited paper were removed. The average number of citations to each author's most-cited item was 453. A study now under way will tell us how many papers in this time period have been cited this many times. Nevertheless, there are some instances where one or two papers account for most citations. The fact is that scientists *of Nobel class* not only publish one or more superstar papers, they also publish more often and their papers achieve higher impact. We observed this phenomenon over 15 years ago in a study of Nobel prizewinners.[8] (p. 63-4)

The world of science is very large. Over one million scientists publish from time to time. Of these, however, a small percentage publish a large percentage of all papers. That is one kind of productivity measure. Of those, however, there is an even smaller number who have a significant impact, which is largely reflected in citations. Since it is generally agreed that there is no significance to a particular citation count, one may usefully think in terms of percentiles—much as we do with IQs or aptitude tests. It would be absurd to choose one doctoral candidate over another because an individual had one point higher on an aptitude test. It

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would be equally absurd to judge anyone on the difference of a few papers or citations. Indeed, it is absurd to use citations, publications, or any other *single* factor alone in evaluations. However, to narrow down one million names to 10,000 or less and then to categorize these names by specialties does provide a useful *beginning* in identifying, in an objective, non-obtrusive manner, those who are making the greatest impact.

Future parts of this series of essays will present a discussion of the 1,000 authors by discipline, provide detailed citation data and publication counts for each author, identify academy memberships and other forms of recognition, and examine the institutions where highly cited research is being conducted.

It is obvious to me that we must extend these initial studies in the future to include at least another 1,000 or 2,000 scientists. I say this if only because the memberships of the world's academies of science exceed this number, and the number of important scientists exceeds that number. But in order to do this in a far more efficient and systematic manner, ISI is developing a statistical data base derived from *SCI* and *Social Sciences Citation Index*® (*SSCI*®) that will facilitate large-scale studies for a variety of scientometric purposes. As we make progress with this system, I'll be reporting the outcome here and in the professional journals. And as the size of the study population increases, we shall identify more people in the smaller or less published fields.

We'd like to thank all those who cooperated with us during this study. In spite of their own publishing deadlines, grant application complications, and other research demands, hundreds of scientists took the time to provide us with information, even though some of these scientists were not included in the study.

* * * * *

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Current Comments®

EUGENE GARFIELD
INSTITUTE FOR SCIENTIFIC INFORMATION®

The 1,000 Most-Cited Contemporary Authors. Part 2A. Details on Authors in the Physical and Chemical Sciences and Some Comments about Nobels and Academy Memberships

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Recently, we published a first look at the 1,000 most-cited contemporary scientists.[1] This part of the continuing series of essays is divided into two sections. First, we will present some statistics on the academy memberships of the entire list of 1,000 authors. In the second section there is a discussion of the 214 authors in the physical and chemical sciences. Future parts of this essay will cover authors in other fields, primarily life sciences. Then a concluding essay will provide a look at the institutional affiliations of the entire 1,000.

It is important to restate the ground rules for this study. It was limited to authors who published journal articles from 1965 to 1978 that were indexed in *Science Citation Index® (SCI®)*, hence the "contemporary" designation. Since data for cited books were not included, certain authors are excluded.

The list was based on "all author" data. Each author of a cited article was treated as a first author regardless of his or her position in the article's by-line. A glance at the data in the physical and chemical sciences section highlights the importance of this point. Generally, authors in this section averaged more cited papers as coauthors than as primary authors. In addition, the majority averaged more citations to coauthored papers than they received to papers on which they were first author.

Only 373 of the 1,000 authors on the list are members of one or more national academies. As a further illustration of the Matthew effect,[2] we found that less than half—161—of these authors are members of just one academy. More than half are members of two or more (123 of two, 48 of three, and 21 of four). Twenty authors belong to more than four academies. The code letter M in Table 1 indicates this fact. These authors are listed along with their memberships in Table 2. A key to their memberships and the number of authors in each academy appear in Table 3. R.B. Woodward, now deceased, was a member of 12, while T. Caspersson is a member of 11.

Of the 1,000 authors listed in Part 1 of this essay,[1] 240 are members of the US National Academy of Sciences (NAS). Seventy-four scientists on the list are members of the Royal Society of London, the equivalent of the UK's national academy. The US Institute of Medicine constitutes a national academy of medicine and has 31 authors on the list.

Less well known to most readers and not regarded as an official academy is the American Academy of Arts and Sciences. It honors men and women for their achievements in the mathematical and physical sciences, biological sciences, social arts and sciences, and the humanities. One hundred ninety-one authors on this list belong to this academy.

We have also included the American Philosophical Society (APS). It also is not, strictly speaking, an academy. Membership is limited to outstanding

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Table 1: The most-cited scientists in the physical and chemical sciences, listed alphabetically by fields. Date of birth is in parentheses. A=total citations. B=first author citations. C=citations as a secondary author. D=total number of cited papers. E=first author papers. F=secondary-authored papers. G=citations/paper. Academy memberships are indicated by a code in column H. A key to these codes appears in Table 3. Asterisks indicate Nobel prizewinners.

Aeronomy

	A	B	C	D	E	F	G	H
AKASOFU SI (1930)	2536	1111	1425	183	69	114	13	
FERGUSON EE (1926)	3445	747	2698	116	34	82	29	

Astronomy

	A	B	C	D	E	F	G	H
GIACCONI R (1931)	2987	905	2082	66	18	48	45	AB
GURSKY H (1930)	3258	401	2857	92	17	75	35	
OKE JB (1928)	2437	1510	927	71	34	37	34	
SALPETER EE (1924)	2441	465	1976	90	19	71	27	ABDF
SANDAGE A (1926)	3997	3309	688	90	65	25	44	A

Astrophysics

	A	B	C	D	E	F	G	H
BAHCALL JN (1934)	2569	2244	325	125	99	26	20	AB
BAME SJ (1924)	3022	474	2548	125	23	102	24	
CAMERON AGW (1925)	2628	970	1658	93	35	58	28	ABN
DALGARNO A (1928)	4127	1760	2367	180	62	118	22	BC
IBEN I (1931)	2489	2154	335	62	45	17	40	
MORTON DC (1933)	2701	1486	1215	70	41	29	38	
OSTRIKER JP (1937)	2469	1117	1352	68	30	38	36	AB

Geophysics

	A	B	C	D	E	F	G	H
FEHSENFELD FC (1934)	3682	1654	2028	129	58	71	28	
LE PICHON X (1937)	2488	1303	1185	49	26	23	50	U

Physics

	A	B	C	D	E	F	G	H
ABRAMS GS (1941)	3249	634	2615	55	14	41	59	
ADLER SL (1939)	3243	3243		62	62		52	AB
ANDERSON PW (1923)*	2795	1545	1250	72	43	29	38	A
BARGER VD (1938)	2913	2873	40	120	117	3	24	
BJORKEN JD (1934)	3199	2847	352	36	30	6	88	AB
BLOEMBERGEN N (1920) *	2438	602	1836	84	25	59	29	ABY!
BOYARSKI AM (1935)	3359	490	2869	51	14	37	65	
BREIDENBACH M (1943)	3789	219	3570	51	5	46	74	
BROWN GE (1926)	2957	1062	1895	111	50	61	26	ABR
BULOS F (1925)	3008	183	2825	38	6	32	79	
CARDONA M (1936)	3190	1061	2129	166	29	137	19	
CHINOWSKY W (1929)	2993	104	2889	41	4	37	73	
COHEN ML (1935)	3882	951	2931	196	36	160	19	A
COWLEY RA (1939)	2888	1127	1761	133	37	96	21	C
CROMER DT (1923)	8778	8121	657	53	27	26	165	

Physics (cont.)

	A	B	C	D	E	F	G	H
RICHTER B (1931) *	3731	54	3677	97	21	76	38	A
RUBBIA C (1934)	2890	32	2858	73	3	70	39	
SAH CT (1932)	2583	919	1664	128	41	87	20	
SATCHLER GR (1926)	3103	1155	1948	89	33	56	34	
SCHMELTEKOPF AL (1932)	2530	378	2152	69	7	62	36	
SCHWITTERS RF (1944)	2938	54	2884	38	5	33	77	
SHIRANE G (1924)	4124	793	3331	178	21	157	23	
SPICER WE (1929)	3433	327	3106	194	19	175	17	
STEWART RF (1936)	5611	4416	1195	68	47	21	82	
TRILLING GH (1930)	3773	39	3734	79	4	75	47	
VAN UITERT LG (1922)	2460	658	1802	161	34	127	15	
VANNUCCI F (1944)	2906	22	2884	35	1	34	83	
VENEZIANO G (1942)	2985	1358	1627	59	13	46	50	
WABER JT (1920)	3566	122	3444	29	5	24	122	
WEINBERG S (1933) *	7687	5711	1976	83	54	29	92	ABC
WHITAKER JS (1948)	2874	24	2850	29	1	28	99	
WILSON KG (1936)	3061	2648	413	47	25	22	65	A
ZUMINO B (1923)	2500	65	2435	35	6	29	71	

Chemical Physics

	A	B	C	D	E	F	G	H
FLYGARE WH (1936)	2790	627	2163	146	21	125	19	AB
SHULMAN RG (1924)	3551	720	2831	111	23	88	31	A

Physical Chemistry

	A	B	C	D	E	F	G	H
BENSON SW (1918)	3334	1627	1707	118	35	83	28	A
BERNSTEIN RB (1923)	3030	591	2439	118	22	96	25	AB
DAVIDSON ER (1936)	5123	314	4809	82	22	60	62	
DUBOIS JE (1920)	2798	1923	875	288	174	114	9	
DURIG JR (1935)	3136	2561	575	259	206	53	12	
FLORY PJ (1910) *	5087	1406	3681	129	46	83	39	ABD
GORDON RG (1940)	3087	1959	1128	76	33	43	40	AB
GRANT DM (1931)	4248	569	3679	106	10	96	40	
HAMILTON WC (1931)	3815	2091	1724	97	28	69	39	
HAYON E (1932)	2523	696	1827	116	26	90	21	
KARPLUS M (1930)	3777	554	3223	110	10	100	34	AB
LEHN J (1939)	2568	854	1714	118	47	71	21	AB

399

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Physics (cont.)	A	B	C	D	E	F	G	H
DRELL SD (1926)	2748	2141	607	41	29	12	67	AB
DUKE CB (1938)	2965	1588	1377	131	73	58	22	
EASTMAN DE (1940)	2891	1596	1295	85	40	45	34	
FELDMAN GJ (1942)	3452	208	3244	63	6	57	54	
FISHER ME (1931)	4604	3085	1519	102	54	48	45	BC
FRIEDBERG CE	2964		2964	33		33	89	
FRYBERGER D (1931)	2851	17	2834	41	3	38	69	
GARITO AF (1939)	2935	228	2707	94	4	90	31	
GIACOMELLI GA (1931)	2483	234	2249	89	22	67	27	
GLASHOW SL (1932) *	3539	1818	1721	41	13	28	86	AB
GOLDHABER G (1924)	3955	446	3509	98	11	87	40	A
GREINER W (1935)	3299	95	3204	162	3	159	20	
GROSS DJ (1941)	2964	2028	936	51	31	20	58	
HANSEN O (1934)	2439	315	2124	132	23	109	18	
HANSON G (1947)	3242	70	3172	56	4	52	57	
HARARI H (1940)	2492	2014	478	52	40	12	47	b
HEEGER AJ (1936)	3450	143	3307	125	6	119	27	
JACKIW R (1939)	2932	1332	1600	60	36	24	48	B
JEAN-MARIE B (1940)	2445	28	2417	32	1	31	76	
JORTNER J (1933)	5449	622	4827	199	20	179	27	b
KADYK JA (1929)	3400	41	3359	57	4	53	59	
LARSEN RR (1929)	3111	16	3095	44	3	41	70	
LEE BW (1935)	3529	1667	1862	86	39	47	41	
LEE TD (1926) *	2868	1859	1009	62	40	22	46	A
LUTH V (1943)	3084	58	3026	47	3	44	65	
LYNCH HL (1939)	2742	51	2691	45	2	43	60	
MANN JB (1923)	2528	290	2238	37	10	27	68	
MAYER JW (1930)	2576	444	2132	149	24	125	17	
MILLER WH (1941)	2889	1676	1213	137	64	73	21	
MOORE CB (1939)	2719	758	1961	125	27	98	21	
MOREHOUSE CC (1942)	2944	23	2921	36	2	34	81	
MOTT NF (1905) *	3128	2348	780	58	43	15	53	ABCM
NESS NF (1933)	2443	1118	1325	92	37	55	26	
PATERSON JM (1937)	3117	6	3111	48	4	44	64	
PERL ML (1927)	3533	527	3006	79	10	69	44	A
PETERLIN A (1908)	3073	1556	1517	162	78	84	18	In
PHILLIPS JC (1933)	2763	1866	897	147	97	50	18	A
PHILLIPS RJ (1930)	2627	772	1855	131	35	96	20	
REEDER DD (1935)	2477	72	2405	60	2	58	41	
REMEIKA JP (1924)	2475	104	2371	154	6	148	16	

Physical Chemistry (cont.)	A	B	C	D	E	F	G	H
LIPSCOMB WN (1919) *	4420	548	3872	179	17	162	24	AB/
MARCUS RA (1923)	2525	1811	714	79	36	43	31	AB
MORINO Y (1908)	2772	1143	1629	104	41	63	26	
POLANYI JC (1929)	2831	816	2015	71	19	52	39	ABCN
RICE SA (1932)	3622	198	3424	177	13	164	20	ABR
SCHERAGA HA (1921)	8062	204	7858	297	12	285	27	AB
SCHMID H (1917)	2674	18	2656	241	5	236	11	F
SHIRLEY DA (1934)	4278	887	3391	177	29	148	24	A
SZWARC M (1909)	2593	173	2420	129	12	117	20	C
WINSTEIN S (1912)	3146	712	2434	93	10	83	33	A
Inorganic Chemistry								
ALLEN LC (1926)	2821	405	2416	66	20	46	42	
CHATT J (1914)	2665	2092	573	145	110	35	18	Cj
CHURCHILL MR (1940)	3633	3164	469	170	149	21	21	
CLARK RJH (1935)	2505	2101	404	108	84	24	23	
COLLMAN JP (1932)	3198	2766	432	82	63	19	39	AB
COTTON FA (1930)	9346	6790	2556	364	256	108	25	ABRX
DAHL LF (1929)	3440	238	3202	108	5	103	31	
DRAGO RS (1928)	3402	690	2712	142	29	113	23	
GRAY HB (1935)	4627	299	4328	192	12	180	24	ABR
GREEN M (1934)	2620	878	1742	191	64	127	13	
GUGGENHEIM HJ (1923)	2673	53	2620	167	3	164	16	
HALPERN J (1925)	2672	1139	1533	119	51	68	22	BC
HAWTHORNE MF (1928)	2748	768	1980	154	30	124	17	AB
HOFFMANN R (1937) *	7340	4170	3170	143	55	88	51	AB
HOLM RH (1934)	2954	324	2630	111	9	102	26	AB
IBERS JA (1930)	7575	208	7367	221	13	208	34	
LEWIS J (1928)	5141	1204	3937	330	60	270	15	C
MASON R (1930)	3750	1103	2647	191	61	130	19	C
MCELROY MB (1939)	2736	1027	1709	89	33	56	30	B
MUETTERTIES EL (1927)	3789	2151	1638	118	56	62	32	AB
MULLER A (1938)	3508	2088	1420	326	193	133	10	
OSBORN JA (1939)	2557	795	1762	45	5	40	56	
PEARSON RG (1919)	2901	2142	759	83	48	35	34	A
SCHRAUZER GN (1932)	2663	2389	274	112	88	24	23	
SHAW BL (1930)	3646	300	3346	159	18	141	22	C
SYMONS MCR (1925)	4593	858	3735	396	94	302	11	
TAUBE H (1915)	2802	158	2644	124	6	118	22	AB
WILLIAMS RJ (1926)	2948	245	2703	200	8	192	14	Cj

Organic Chemistry

	A	B	C	D	E	F	G	H
ALLINGER NL (1928)	3023	2594	429	116	87	29	26	
BAKER BR (1915)	2657	2559	98	191	184	7	13	
BARTON DHR (1918) *	3177	2646	531	255	196	59	12	ABCM
BATTERSBY AR (1925)	2564	2290	274	134	119	15	19	CF
BENDER ML (1924)	3118	1512	1606	115	34	81	27	A
BÖHLMANN F (1921)	2640	2360	280	412	369	43	6	
BOWIE JH (1938)	2627	2036	591	154	110	44	17	
BROWN HC (1912) *	8756	6723	2033	393	271	122	22	ABCZ
BRUICE TC (1925)	2876	805	2071	160	37	123	17	AB
COREY EJ (1928)	9152	8662	490	273	248	25	33	AB
CRAM DJ (1919)	2849	733	2116	137	22	115	20	AB
DJERASSI C (1923)	7704	850	6854	378	32	346	20	ABEM
ELIEL EL (1921)	2981	2313	668	94	67	27	31	A
FOLKERS K (1906)	2999	283	2716	203	17	186	14	A
GASSMAN PG (1935)	2997	2954	43	159	156	3	18	
HAMMOND GS (1921)	2858	94	2764	106	13	93	26	AKY
HANSCH C (1918)	3926	2289	1637	110	60	50	35	
HEILBRONNER E (1921)	3156	765	2391	145	40	105	21	BX
HOUSE HO (1929)	2914	2626	288	88	83	5	33	
HUISGEN R (1920)	5087	3716	1371	203	126	77	25	BFWX
INGOLD KU (1929)	2619	124	2495	132	6	126	19	CN
JENCKS WP (1927)	3527	836	2691	110	19	91	32	AB
JERINA DM (1940)	5033	1235	3798	155	21	134	32	
JOHNSON BFG (1938)	3045	1360	1685	204	84	120	14	
KATRITZKY AR (1928)	4385	1043	3342	360	82	278	12	C
KHORANA HG (1922) *	4409	311	4098	131	8	123	33	ABCM
KUPCHAN SM (1922)	2997	2681	316	173	152	21	17	
MISLOW K (1923)	3202	334	2868	154	11	143	20	AB
NAKANISHI K (1925)	2593	322	2271	213	18	195	12	B
NETA P (1938)	2460	870	1590	74	37	37	33	
NOZAKI H (1922)	2680	999	1681	245	61	184	10	
OLAH GA (1927)	7910	7190	720	403	362	41	19	A
PAQUETTE LA (1934)	5116	4493	623	345	283	62	14	
RAMIREZ F (1923)	2999	1882	1117	144	98	46	20	
ROBERTS JD (1918)	5941	92	5849	201	6	195	29	ABD
ROBINS RK (1926;	4157	64	4093	241	3	238	17	
SCHLEYER PVR (1930)	5736	1281	4455	189	28	161	30	
SORM F (1913)	4890	119	4771	382	10	372	12	ABFM
SWEELEY CC (1930)	2851	361	2490	84	8	76	33	
TROST B (1941)	3087	2321	766	142	121	21	21	A
TURRO NJ (1938)	3101	1906	1195	143	80	63	21	A
WENKERT E (1925)	2637	1369	1268	142	71	71	18	K
WHITESIDES GM (1939)	2625	1697	928	79	41	38	33	AB
WIBERG KB (1927)	2769	2582	187	106	95	11	26	A
WILLIAMS DH (1937)	5591	1107	4484	203	38	165	27	
WITKOP B (1917)	3921	82	3839	169	6	163	23	AF
WOODWARD RB (1917) *	3644	2360	1284	49	23	26	74	ABCM
YAGI H (1939)	2841	291	2550	147	16	131	19	
ZIMMERMAN HE (1926)	3226	3088	138	113	111	2	28	A

Organometallic Chemistry

	A	B	C	D	E	F	G	H
CLARK HC (1929)	3026	1855	1171	139	87	52	21	N
FISCHER EO (1918) *	3472	2288	1184	250	163	87	13	BFIM
HASZELDINE RN (1925)	2684	414	2270	300	51	249	8	C
KING RB (1938)	4467	3562	905	262	211	51	17	
KOCHI JK (1928)	4084	1684	2400	164	39	125	24	
LAPPERT MF (1928)	3321	727	2594	206	58	148	16	C
SCHMIDBAUR H (1934)	2576	2023	553	197	149	48	13	
SEYFERTH D (1929)	3749	3510	239	233	215	18	16	F
STONE FGA (1925)	4013	39	3974	239	4	235	16	C
WILKINSON G (1921) *	5444	58	5386	167	8	159	32	ABCM

Analytical Chemistry

	A	B	C	D	E	F	G	H
BEROZA M (1917)	2474	807	1667	168	40	128	14	
COOKS RG (1941)	2594	673	1921	143	34	109	18	
MCLAFFERTY FW (1923)	3018	1087	1931	117	32	85	25	
WEST TS (1927)	3214	257	2957	222	11	211	14	S
WINEFORDNER JD (1931)	2811	334	2477	212	22	190	13	

Theoretical Chemistry

	A	B	C	D	E	F	G	H
CLEMEMTI E (1931)	4001	3011	990	76	34	42	52	
DEWAR MJS (1918)	8368	5578	2790	255	191	64	32	BC
HEHRE WJ (1945)	4856	2352	2504	85	34	51	57	
LEVINE RD (1938)	2676	1075	1601	128	47	81	20	
POPLE JA (1925)	12714	6351	6363	131	25	106	97	ABC
SCHAEFER HF (1944)	3166	697	2469	146	23	123	21	
SEGAL GA (1934)	4159	258	3901	24	7	17	173	
SIMPSON WT (1920)	4316	13	4303	25	4	21	172	
STEPHENS FS (1931)	2789	1094	1695	120	38	82	23	

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401

Table 2: Authors on 1,000 most-cited list who are members of more than four academies.

Anfinsen C B (5) ABDRb
Barton D H R (8) ABCDLRSp
Brenner S (5) ABCDF
Caspersson T (11) BCDFJKTYhlq
Chance B (7) ABDFGWl
De Duve C (6) ABFJUi
Djerassi C (7) ABEFKel
Fisher E O (5) BFIWX
Jacob F (7) ABCDERU
Katchalski-Katzir E (6) ABCDFb
Katz B (5) ABCRc
Khorana H G (7) ABCDFYr
Krebs H A (7) ABCDFWX
Lipmann F (5) ACDFR
Mott N F (5) ABCFX
Ochoa S (10) ABCDFZhikr
Palade G E (7) ABEFJis
Perutz M F (9) ABCDFISUf
Sorm F (10) ABFLPRhprs
Woodward R B (12) ABCDFIYZacnr

contributors to the mathematical and physical, geological, and biological sciences. Twenty-four APS members are on the list.

Thirty-nine authors belong to the Deutsche Akademie der Naturforscher Leopoldina, the national academy of the German Democratic Republic. The Federal Republic of Germany does not have a national academy; five state academies serve this purpose. The academies in the states of Bavaria and Göttingen have four and five members, respectively, while Heidelberg has one member listed. There are in fact 45 different academies listed.

When we did our study of the 300 most-cited authors 1961-1976, 26 Nobelists were identified.[3] Seven more on that list have since won the prize, including three of this year's winners.[1] By comparison, there are only 22 Nobelists among the top 300 in our new list of 1,000 names.

Three Nobelists on the earlier list of 300 do not appear on the new 1,000 author list. This emphasizes the chronological "bias" in these studies. Since much of the work involved in past Nobel prizes was published prior to 1965, it is remarkable that so many names remain. Thus, work *of Nobel class* generally has long-lived impact.[4]

In case you are wondering why some relatively contemporary Nobelists do not appear on this list, keep in mind that

Table 3: The academies of the 1,000 authors, including the number of authors from each academy.

A	=	National Academy of Sciences, US	240
B	=	American Academy of Arts and Sciences	191
C	=	Royal Society of London, UK	74
D	=	American Philosophical Society	24
E	=	Institute of Medicine, US	31
F	=	Deutsche Akademie der Naturforscher Leopoldina, DDR	39
G	=	National Academy of Sciences of Argentina	4
H	=	Australian Academy of Science	7
I	=	Austrian Academy of Sciences	5
J	=	Royal Academy of Sciences, Letters and Fine Arts of Belgium	7
K	=	Brazilian Academy of Sciences	5
L	=	Bulgarian Academy of Science	4
M	=	More than four academy memberships (see Table 2)	20
N	=	Royal Society of Canada	14
O	=	Academy of Sciences of Chile	1
P	=	Czechoslovakian Academy of Sciences	2
R	=	Royal Danish Academy of Sciences and Letters	16
S	=	Royal Society of Edinburgh, UK	8
T	=	Academy of Finland	2
U	=	Academy of Sciences of France	10
V	=	Academie Francaise	1
W	=	Bavarian Academy of Sciences, FRG	4
X	=	Göttingen Academy, FRG	5
Y	=	Indian Academy of Sciences, Bangalore	5
Z	=	Indian National Science Academy, New Delhi	4
a	=	Royal Irish Academy	2
b	=	Israel Academy of Sciences and Humanities	6
c	=	Lincei National Academy, Italy	4
d	=	Japan Academy	2
e	=	National Academy of Sciences of Mexico	1
f	=	Royal Netherlands Academy of Sciences and Letters	7
g	=	Norwegian Academy of Science and Letters	1
h	=	Polish Academy of Sciences	4
i	=	Pontifical Academy of Sciences	6
j	=	Lisbon Academy of Sciences, Portugal	2
k	=	Royal Spanish Academy	2
l	=	Royal Swedish Academy of Sciences	14
m	=	Academia Sinica, Taiwan	1
n	=	Slovene Academy of Arts and Sciences, Yugoslavia	2
o	=	Serbian Academy of Sciences and Arts, Yugoslavia	1
p	=	Hungarian Academy of Sciences	2
q	=	Academy of Sciences of Venezuela	1
r	=	Academy of Sciences of the USSR	4
s	=	Academy of the Socialist Republic of Romania	2
t	=	Heidelberg Academy of Sciences, FRG	1
u	=	Yugoslav Academy of Sciences and Arts	1

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Classification of Authors' Research Specialties

The contention of sociologist Warren O. Hagstrom, University of Wisconsin at Madison, that "resistance to classification is quite general among scientists"[1] became clear to us while working on this project.

After we sent reprints of the first essay in this series to the 1,000 authors, we heard from several that they wished to be reclassified.

Our questionnaire was probably at fault in most instances of misclassification. We asked authors to pick from a list of 30 general disciplinary areas. Many questionnaires were returned with a single category chosen, but others came back with up to five disciplines checked. In these cases, our research staff had to choose one—based on information in directories such as *American Men and Women of Science*[2] and on the journals in which these authors published. Sometimes this led to a choice not preferred by the author.

The questionnaire also erred in not offering enough disciplinary areas. For instance, we did not include analytical chemistry. This led to a dilemma for analytical chemists who thought they had to choose another category. Fred W. McLafferty, Cornell University, an analytical chemist who was misclassified on the list, was mentioned by name in a *Chemical & Engineering News* article on the study.[3] He wrote to tell us that he has taken a bit of kidding over his classification as an organic chemist. He told us, "It seems [the organic chemists] don't want me."[4] He and four others have been reclassified as analytical chemists. The others are M. Beroza, R. G. Cooks, T. S. West, and J. D. Winefordner.

Other authors on the list who have been reclassified are: L. F. Dahl, from physical chemistry to inorganic chemistry; M. J. S. Dewar, from physical chemistry to theoretical chemistry; J. R. Durig, from inorganic chemistry to physical chemistry; R. D. Levine, from physics to theoretical chemistry; A. Muller, from physics to inorganic chemistry; P. V. R. Schleyer, from physical chemistry to organic chemistry; and G. A. Segal, from physics to theoretical chemistry.

In general, we have only changed categories at the author's request. In the cases in which one author has written to criticize another's classification, we checked our files, and if the scientist involved has clearly indicated a discipline and has *not* asked to be moved, we made no changes.

In one case, an author was so vehement in his denunciation of our misclassifying a colleague that we called the scientist in question. It turned out that he had simply checked off the wrong discipline when he filled out the questionnaire.

Despite this explanation, the "misclassification" problem is not a large one. We have heard from nearly 300 of the 1,000 authors on the list, and less than 25 presented problems.

We have learned from this exercise that we must refine our survey techniques in the future—perhaps by listing more categories or by asking an open-ended question like, "What kind of scientist are you, anyway?"

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a Nobel prize is the recognition of a field as much as an accolade to particular individuals. Some fields are recognized in which there are relatively small numbers of researchers. So when the prize was given to A.A. Penzias and R.W. Wilson for their work in radio astronomy it was not surprising that they did not appear in a list dominated by life scientists.[4] However, when we examined the citation record for the cluster of authors turning up in the research front for radio astronomy, they were among the five most-cited authors.

As I've stated before, different fields or disciplines have different citation and publication patterns. Some of the data presented here indicate some of these differences, though I would not want to make any claims for the statistical valid-

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ity where small samples for a field are represented.

Do not attribute special significance to trivial differences in citation counts of the individual authors listed. Some authors may be associated with highly cited methods not regarded as significant breakthroughs but almost all are *of Nobel class*.[4] Keep in mind that even a list of 1,000 authors only accounts for .2 percent of publishing scientists in the world.[5] Therefore, we have most likely quite arbitrarily excluded an even larger number of equally important scientists. I'm sure that most members of the various academies would agree that there are at least an equal number of highly qualified scientists who are excluded from membership in national academies.

In the list of authors in the physical and chemical sciences (see Table 1), authors are listed alphabetically beneath their disciplines. To accurately depict their disciplines, the authors themselves were asked to name the categories they preferred. (See accompanying box for further explanation.) For each author the following information is provided—citations received, citations as a first author, citations as a subsequent author, total number of papers cited, papers as a primary author, papers as a secondary author, and citation rate.

A citation "rate" is the average number of citations per cited paper. For example, M. L. Cohen's 196 papers received 3,882 citations. The rate of citation is 19. On the other hand, J. S. Whitaker received 2,874 citations. Only 29 of his papers were involved, giving a citation rate of 99. The significance of this number varies considerably, especially when one or two highly cited papers account for most of the citations.

Academy memberships are noted by a code at the end of each entry. The codes are explained in detail in Table 3. Nobelists are indicated by asterisks.

This essay covers data for authors in the fields of aeronomy (a branch of geophysics that deals with the atmosphere of Earth and other celestial bodies), astronomy, astrophysics, geophysics, physics, chemical physics, physical chemistry, inorganic chemistry, organic chemistry, organometallic chemistry, analytical chemistry, and theoretical chemistry.

As you can see from Table 4, the average number of citations varies among disciplines. Theoretical chemistry received the highest average number of citations while analytical chemistry received the lowest. The average number of cited papers also differs among fields. For example, authors in organometallic chemistry published cited articles more often than did authors in other fields. Astronomers on the list published cited articles less often than their peers in other disciplines, averaging 82 cited papers during this time period. Harriet Zuckerman points out that this "low" is still an astonishingly high rate of cited work.[6] Publishing practices also varied. In one field, astrophysics, authors received slightly more citations as first authors than they did as secondary authors. In every other field, authors were more often cited as secondary authors. Of course, whether or not an author is cited more often as a first author could be the result of alphabetic listing popular in some journals and fields.

The organometallic chemists, physical chemists, and organic chemists were the oldest among the physical and chemical scientists. The geophysicists and the physicists were the youngest. We also observed that the average age of academy members on the list of 1,000 was 58, while the average age of all authors on the list of 1,000 was 53, and the average age of non-academy members was 51. More than half of the organic chemists (who were older than average) belonged to academies, while only about a quarter of the physicists (who were much younger) were academy members.

Sidney Drell, Stanford Linear Accelerator Center, observes that 23 of the

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Table 4: Discipline averages for authors in physical and chemical sciences. A = number of authors on list. B = average number of citations received. C = average primary citations. D = average secondary citations. E = average number of cited papers. F = average papers as first author. G = average papers as secondary author. H = number of authors with academy memberships. I = number of Nobelists. J = average birth year.

Discipline Name	A	B	C	D	E	F	G	H	I	J
Aeronomy	2	2990	929	2061	149	51	98	—	—	1928
Astronomy	5	3024	1318	1706	82	31	51	3	—	1928
Astrophysics	7	2858	1458	1400	103	48	55	4	—	1930
Geophysics	2	3085	1478	1606	89	42	47	1	—	1935
Physics	73	3254	1056	2198	85	25	60	22	7	1935
Chemical physics	2	3170	673	2497	128	22	106	2	—	1930
Physical chemistry	22	3611	984	2627	144	39	105	15	2	1925
Inorganic chemistry	28	3759	1448	2311	166	58	108	16	1	1929
Organic chemistry	49	3838	1861	1977	185	85	100	30	4	1926
Organometallic chemistry	10	3684	1616	2068	216	98	117	7	2	1927
Theoretical chemistry	9	5227	2270	2957	110	45	65	2	—	1932
Analytical chemistry	5	2822	631	2191	172	28	144	1	—	1928

authors on the physics portion of the list all worked together on a "fantastically important and successful collaboration"[7] at the center's electron-positron storage ring. That collaboration resulted in a 1974 paper published in *Physical Review Letters* entitled "Discovery of a narrow resonance in $e^+ e^-$ annihilation."[8] One of the authors listed on that paper, B. Richter, won his Nobel prize for work in this area. He and G. Goldhaber, another author on that paper, both have published more papers than the other collaborators. Interestingly, ten of those 23 authors who made the top 1,000 would not have appeared on the list were it not for the 626 citations which that single paper received.

This example illustrates the difficulties in assigning credit to multiauthored works. Some would agree with Derek J. de Solla Price, who argues that it is absurd to give all authors on a large team equal credit to that received by someone who is sole author of a highly cited paper. In a letter to *Science*,[9] Price suggested that perhaps credit should be assigned *proportionately* to each author, that is, a citation to a paper with two authors means that both receive credit for one-half of a citation. Three authors of a single paper would receive credit for one-third of all citations to that paper, and so on. Had we used this method of counting, one-quarter of the physicists in this study would not have made the list. A similar drop-off might occur for other fields also, but this is more properly a subject for future essays. Our main purpose was to eliminate the shortcomings of studies based on first-authorship only.

Since we had to limit the size of this study we have not been able to list the many other prestigious awards received by these authors. We intend to do this in a separate study of the "unNobel prizes." Not the least of these are the various National Medal of Science winners such as astronomer A. R. Sandage and physical chemist Saul Winstein.

Clearly it would have been interesting to study the educational origins of this group, perhaps to determine how many were taught by other Nobelists or to determine which educational institutions produced these productive scientists.

I've often been asked why we gather data on prizes won by highly cited authors. There can be no doubt that there is something defensive about this activity. There is great controversy about the use of citation or publication counting in evaluating scientists. The whole process of so-called peer review or evaluation is a very sensitive issue. Can anyone ever

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put a number on the worth of scientific discovery? There are those who reasonably argue that trying to measure the significance of individual scientific work is as impossible as measuring beauty. It is an inherently subjective process. As long as we recognize that the procedures in electing people to academies are inherently subjective and political, we need not kid ourselves that the procedures involved are infallible. I cannot stress too often that citation analysis is a measure of impact in the literature and an imperfect one at that. But the size of the scientific enterprise is so large that we need better and more objective indicators of merit at those times when we wish to exercise collective subjective judgments. That's what a committee does.

I wish we could afford the time and space to discuss the achievements of each scientist identified in this study. That is ultimately the purpose of our *Atlas of Science*[10] or our *Citation Classics* series.[11] We didn't even have the space to mention the fields they have pioneered, most of which turn up in the continuing series of research fronts we publish for use with our new online facilities.[12,13]

In the next part of this series, I will begin a discussion of authors in the life sciences including biochemistry, molecular biology, biophysics, cell biology, enzymology, genetics, and plant sciences.

* * * * *

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Current Comments®

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The 1,000 Most-Cited Contemporary Authors. Part 2B. Details on Authors in Biochemistry, Biophysics, Cell Biology, Enzymology, Genetics, Molecular Biology, and Plant Sciences

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In this essay, we continue our study of the 1,000 most-cited contemporary scientists. In Part 1 we provided the entire list of authors.[1] Part 2A examined data for 214 authors in the physical and chemical sciences.[2] This essay covers 267 authors in the first of three groups of life sciences disciplines. The remaining life scientists will be covered in two more essays to follow. The next part will cover immunology, virology, microbiology, physiology, histology, and hematology. The final part will cover 14 other life and clinical sciences disciplines.

The term "contemporary authors" is used because the citation data culled for this analysis were limited to articles published from 1965 to 1978, and indexed in *Science Citation Index*® (*SCI*®). Data for cited books were not included. The list was produced from "all-author" data, meaning that each author was treated as a first author, regardless of his or her position in an article's by-line.

The disciplines included in this essay are biochemistry, biophysics, cell biology, enzymology, genetics, molecular biology, and plant sciences. Classifying scientists by traditional disciplines is a real challenge, especially in the life sciences. A recent book which defines the various branches of biology makes this clear. As Joshua Lederberg writes in the genetics section of the book: "The science of living things is too complicated both in method and in objective to yield to tidy classification."[3] He goes on to note that genetics is "a particular way of looking at almost every aspect of biology." The same can perhaps be said for many life sciences disciplines.

Another reason it is often difficult to classify scientists is that many of them work in more than one discipline. And sometimes a scientist will start in a discipline like physics and then "cross over" into molecular biology. Thus, while a particular scientist may have accumulated a massive citation record in one field, he or she may be treated in this study as part of another discipline. Quite often, scientists are ambivalent about their classifications. If a molecular biologist is using the techniques of X-ray crystallography, he or she may have divided loyalties. For example, M. Sundaralingam, University of Wisconsin, chose to be listed here as a biophysicist, but he also considers himself an X-ray crystallographer.

We allowed the authors in this study to classify themselves by checking the appropriate box in a list of specialties included in a questionnaire. Not surprisingly, we found considerable disciplinary overlap. For example, many authors checked biochemistry as well as other disciplines, sometimes as many as three or four. In cases where authors checked more than one box, we used other methods to "pigeonhole" them. Since the publication of the original list of 1,000 authors, two of the authors listed

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Table 1: The most-cited scientists in the preclinical basic sciences (first group), listed alphabetically by fields. Date of birth is in parentheses. A=total citations. B=first author citations. C=citations as a secondary author. D=total number of cited papers. E=first author papers. F=secondary-authored papers. G=citations/paper. Academy memberships are indicated by a code in column H. A key to these codes appears in Table 2. Asterisks indicate Nobel prizewinners.

Biochemistry

	A	B	C	D	E	F	G	H
ALLFREY VG (1921)	4196	218	3978	73	7	66	57	
ANDREWS P (1928)	2515	2245	270	50	26	24	50	
ANFINSEN CB (1916) *	4343	288	4055	92	10	82	47	ABDM
ANTONINI E (1931)	3127	962	2165	166	29	137	18	
ATKINSON DE (1921)	3301	2341	960	44	14	30	75	
AXEN R (1930)	2573	1577	996	28	11	17	91	
BENESCH R (1919)	2870	2003	867	62	32	30	46	
BENESCH RE (1925)	2742	570	2172	43	17	26	63	
BROWN MS (1941)	3188	1430	1758	119	45	74	26	A
CANTOR CR (1942)	2616	541	2075	75	16	59	34	
CASIDA JE (1929)	2445	340	2105	149	13	136	16	
CHANCE B (1913)	7131	2756	4375	286	94	192	24	ABDM
CHRAMBACH A (1927)	2744	1321	1423	80	13	67	34	
CUATRECASAS P (1936)	10543	7060	3483	179	59	120	58	
DAWSON RMC (1924)	2477	669	1808	91	27	64	27	
DELUCA HF (1930)	12090	998	11092	323	33	290	37	AB
EDELHOCH H (1922)	2644	1359	1285	94	19	75	28	
ERNSTER L (1920)	3592	508	3084	87	10	77	41	I
ESTABROOK RW (1926)	4314	274	4040	87	13	74	49	A
EYLAR EH (1934)	3293	1002	2291	79	18	61	41	
FASMAN GD (1925)	4228	531	3697	95	14	81	44	
GELBOIN HV (1929)	4169	931	3238	93	17	76	44	
GOLDSTEIN IJ (1929)	3201	1241	1960	90	17	73	35	
GOODWIN TW (1916)	2543	171	2372	156	13	143	16	C
GROSS J (1917)	2546	17	2529	67	2	65	38	AB
HAMBERG M (1944)	4915	3579	1336	85	40	45	57	
HENDERSON JF (1933)	2605	723	1882	123	41	82	21	
HILL RL (1928)	2919	282	2637	98	6	92	29	ABE
HORECKER BL (1914)	3190	142	3048	129	9	120	24	ABK
HUISMAN THJ (1923)	3082	1314	1768	154	42	112	20	
JACKSON RL (1939)	2784	887	1897	103	46	57	27	
JOHNSON GS (1943)	2557	988	1569	43	20	23	59	
KIVIRIKKO KI (1937)	2501	1220	1281	77	19	58	32	

Biochemistry (cont.)

	A	B	C	D	E	F	G	H
TANFORD C (1921)	5924	1252	4672	91	20	71	65	AB
TAPPEL AL (1926)	4258	566	3692	134	12	122	31	
TATA JR (1930)	2837	1351	1486	67	33	34	42	C
TOMKINS GM (1926)	7252	902	6350	112	10	102	64	
UDENFRIEND S (1918)	8641	1211	7430	162	10	152	53	AB
VAGELOS PR (1929)	2933	133	2800	96	3	93	30	ABE
VALLEE BL (1919)	4829	651	4178	159	14	145	30	ABR
VAN DEENEN LL (1928)	8267	274	7993	214	10	204	38	BI
VAUGHAN M (1926)	2572	231	2341	78	7	71	32	
WALSH DA (1939)	2490	1170	1320	37	16	21	67	
WEBER K (1936)	13427	10402	3025	137	45	92	98	
WESTPHAL OH (1913)	3129	53	3076	104	9	95	30	FI
WILLIAMSON JR (1933)	2958	1925	1033	81	39	42	36	
WILSON DF (1938)	2713	1416	1297	115	59	56	23	

Molecular Biology

	A	B	C	D	E	F	G	H
ATTARDI G (1926)	3294	937	2357	80	12	68	41	
BARRELL BG (1944)	2898	297	2601	33	5	28	87	
BERG P (1926) *	3411	152	3259	79	11	68	43	AE
BERNARDI G (1929)	2438	1211	1227	86	26	60	28	
BORISY GG (1942)	3110	1471	1639	45	10	35	69	
BORST P (1934)	2661	879	1782	99	16	83	26	I
BRAWERMAN G (1927)	2623	636	1987	39	10	29	67	
BRENNER S (1927)	2611	539	2072	49	11	38	53	ABCM
BRITTEN RJ (1919)	4360	2348	2012	51	10	41	85	A
BROWN DD (1931)	3218	1599	1619	76	35	41	42	AB
BROWNLEE GG (1942)	2882	1355	1527	32	12	20	90	
CHALKLEY R (1939)	3742	180	3562	65	4	61	57	
CHAMBON PH (1931)	3397	767	2630	71	18	53	47	U
CLARK AJ (1933)	2853	1000	1853	65	22	43	43	
COHEN SN (1935)	2884	1226	1658	90	36	54	32	AB

Biochemistry (cont.)	A	B	C	D	E	F	G	H
KODICEK E (1908)	2623	194	2429	71	6	65	36	C
KOSHLAND DE (1920)	5208	1124	4084	111	11	100	46	AB
KREBS EG (1918)	4578	86	4492	61	1	60	75	AB
KREBS HA (1900) *	4670	1079	3591	83	25	58	56	ABCM
LANDS WEM (1930)	2441	713	1728	72	15	57	33	
LARDY HA (1917)	5064	617	4447	128	8	120	39	ABD
LEHNINGER AL (1917)	3447	837	2610	92	15	77	37	ABDF
LEONARD NJ (1916)	3080	1004	2076	148	56	92	20	ABn
LI CH (1913)	4773	1522	3251	249	62	187	19	ABDn
LINNANE AW (1930)	2916	357	2559	78	8	70	37	CH
LIPMANN F (1899) *	3173	265	2908	56	10	46	56	ACDM
LOWRY OH (1910)	2570	439	2131	74	8	66	34	ABR
MEISTER A (1922)	3691	447	3244	163	14	149	22	ABE
MILLER EJ (1935)	3140	1915	1225	79	30	49	39	
MITCHELL PD (1920) *	3537	2913	624	60	32	28	58	ABC
MUNRO HN (1915)	4299	833	3466	145	30	115	29	AS
NORMAN AW (1938)	3432	962	2470	142	26	116	24	
OCHOA S (1905) *	2462	33	2429	61	3	58	40	ABCM
OVCHINNIKOV YA (1934)	2458	516	1942	165	34	131	14	FL
PASTAN IH (1931)	8090	1666	6424	171	34	137	47	
PIEZ KA (1924)	3067	615	2452	48	15	33	63	
PORATH JO (1921)	3349	1036	2313	58	15	43	57	I
PORTER JW (1915)	2619	168	2451	130	17	113	20	
PROCKOP DJ (1929)	5555	608	4947	136	11	125	40	
RACKER E (1913)	6206	1251	4955	143	26	117	43	AB
REICH E (1927)	2753	134	2619	76	9	67	36	
ROEDER RG (1942)	2748	1454	1294	29	8	21	94	
ROSEMAN S (1921)	4377	661	3716	92	4	88	47	AB
SAMUELSSON B (1934)	7377	996	6381	140	23	117	52	
SATO R (1923)	3388	183	3205	169	58	111	20	
SHARON N (1925)	3699	906	2793	130	16	114	28	
SJOVALL J (1928)	2814	282	2532	110	9	101	25	
SMITH EL (1911)	3812	574	3238	163	43	120	23	ABD
SNYDER F (1931)	3172	1041	2131	171	46	125	18	
SPIRO RG (1929)	3258	2350	908	54	29	25	60	
STADTMAN ER (1919)	2636	305	2331	78	7	71	33	AB
STECK TL (1939)	4457	1510	2947	38	15	23	117	
STEINBERG D (1922)	3025	652	2373	123	28	95	24	

Molecular Biology (cont.)	A	B	C	D	E	F	G	H
DATTA N (1922)	2796	1063	1733	64	22	42	43	
DAVIDSON EH (1937)	3334	1180	2154	61	19	42	54	
DAVIDSON NR (1916)	4702	314	4388	123	17	106	38	A
DOTY PM (1920)	2762	3	2759	48	1	47	57	ABDs
FELSENFELD G (1929)	3200	460	2740	36	5	31	88	A
FLEISCHER S (1930)	2925	629	2296	64	10	54	45	
GALL JG (1928)	2465	1328	1137	39	19	20	63	AB
GOODMAN HM (1938)	2829	486	2343	81	5	76	34	
GREEN M (1926)	3554	988	2566	166	25	141	21	
GROS F (1925)	2491	29	2462	104	4	100	23	U
HARTLEY BS (1926)	3209	1104	2105	40	6	34	80	C
HURWITZ J (1928)	3297	288	3009	80	6	74	41	AB
JACOB F (1920) *	3603	340	3263	106	10	96	33	ABCM
KABACK HR (1936)	3333	1371	1962	85	20	65	39	
KLUG A (1926)	3308	522	2786	85	19	66	38	BC
KORNBERG A (1918) *	5275	445	4830	103	9	94	51	AC
KURLAND CG (1936)	2548	483	2065	44	6	38	57	
LAEMMLI UK (1940)	5148	4790	358	20	11	9	257	
LEDER P (1934)	3980	657	3323	73	17	56	54	A
LEHMANN H (1910)	3780	548	3232	284	56	228	13	C
LOENING UE (1931)	3626	3031	595	25	10	15	145	
MAIZEL JV (1934)	5596	1025	4571	53	6	47	105	
MONOD JL (1910) *	2973	2301	672	17	3	14	174	A
NEVILLE DM (1934)	3821	1192	2629	38	8	30	100	
NISHIMURA S (1931)	2638	531	2107	113	8	105	23	
NOMURA M (1927)	5174	1408	3766	121	18	103	42	ABR
PAPAHADJOPOULOS DP (1934)	3496	2135	1361	67	31	36	52	
PAUL J (1922)	3541	1228	2313	115	36	79	30	S
PENMAN S (1930)	7539	1841	5698	105	11	94	71	
PERRY RP (1931)	3577	2159	1418	69	28	41	51	A
PERUTZ MF (1914) *	4921	4003	918	61	34	27	80	ABCM
PHILLIPS DC (1924)	2481	527	1954	42	11	31	59	BC
PORTER KR (1912)	2635	446	2189	65	18	47	40	AB
RABINOWITZ M (1927)	2532	632	1900	126	28	98	20	
RICH A (1924)	4811	236	4575	166	17	149	28	AN
RICHARDSON CC (1935)	3078	750	2328	64	6	58	48	B
RUTTER WJ (1928)	4481	330	4151	95	4	91	47	

409

Molecular Biology (cont.)	A	B	C	D	E	F	G	H
SANGER F (1918) *	3194	1418	1776	29	10	19	110	ABC
SCHIMKE RT (1932)	4810	1143	3667	73	12	61	65	AB
SCHLESSINGER D (1936)	2695	352	2343	104	11	93	25	
SHARP PA (1944)	2693	1205	1488	42	10	32	64	
SIMS P (1920)	4617	1147	3470	100	24	76	46	
SINSHEIMER RL (1920)	4162	123	4039	118	7	111	35	ABE
SPIEGELMAN S (1914)	8415	953	7462	131	20	111	64	AB
STUDIER FW (1936)	4203	3467	736	23	12	11	182	
SZYBALSKI W (1921)	2890	327	2563	76	10	66	38	
VINOGRAD J (1913)	4185	824	3361	63	8	55	66	A
WEISSBACH H (1932)	3395	563	2832	154	23	131	22	
WITTMANN HG (1927)	2776	333	2443	83	15	68	33	BF
WYMAN J (1901)	4133	229	3904	71	8	63	58	ABc
YANOFSKY C (1925)	4654	627	4027	130	13	117	35	ABF
ZINDER ND (1928)	2528	162	2366	72	5	67	35	AB

Biophysics

ALLERHAND A (1937)	2608	1565	1043	67	30	37	38	
BERNHARD W (1920)	2381	986	1895	61	17	44	47	
BLOW DM (1931)	2465	735	1730	26	10	16	94	C
BUTLER WL (1925)	2454	675	1779	85	19	66	28	AB
CHAPMAN D (1927)	4404	1442	2962	124	44	80	35	
CURRAN PF (1931)	2801	337	2464	64	9	55	43	
GREEN DE (1910)	3507	1497	2010	135	53	82	25	ABJ
KARLE IL (1921)	2872	1107	1765	103	57	46	27	A
KATCHALSKI-KATZIR E (1916)	2613	17	2596	72	2	70	36	ABCM
KATZ B (1911) *	3049	2969	80	50	41	9	60	ABCM
KEARNS DR (1935)	3695	1126	2569	124	22	102	29	
KLINGENBERG ME (1928)	2548	479	2069	73	18	55	34	
MCCONNELL HM (1927)	5697	368	5329	102	12	90	55	AB
MILEDI R (1927)	5059	1654	3405	93	30	63	54	C
PACKER L (1929)	2650	654	1996	117	34	83	22	
REYNOLDS JA (1930)	2548	1274	1274	67	29	38	38	
SETLOW RB (1921)	2879	1122	1757	75	24	51	38	AB
SHAPIRO AL (1930)	2999	2955	44	15	9	6	199	
SMALL DM (1931)	3322	1020	2302	109	21	88	30	

Cell Biology (cont.)	A	B	C	D	E	F	G	H
KARNOVSKY MJ (1926)	11427	3199	8228	124	12	112	92	B
KIPNIS DM (1927)	5676	223	5453	109	6	103	52	ABE
KORN ED (1928)	2480	1020	1460	67	21	46	37	
LODISH HF (1941)	3517	1817	1700	90	35	55	39	
MAHLER HR (1921)	2528	421	2107	100	23	77	25	
MANDEL P (1908)	4966	311	4655	424	32	392	11	
MATHE G (1922)	3951	2544	1407	276	149	127	14	C
MEANS AR (1941)	2555	882	1673	86	24	62	29	
MELMON KL (1934)	3427	642	2785	133	21	112	25	BE
MIRSKY AE (1900)	2458	243	2215	29	6	23	84	ADF
NICOLSON GL (1943)	6047	3245	2802	77	39	38	78	
NORTHCOTE DH (1921)	2945	613	2332	91	11	80	32	C
OSBORN M (1940)	10376	501	9875	38	13	25	273	
PALADE GE (1912) *	7915	277	7638	96	5	91	82	ABEM
PARDEE AB (1921)	3110	1117	1993	70	18	52	44	ABE
POTTER VR (1911)	3754	321	3433	99	15	84	37	AB
RAFF MC (1938)	4499	2502	1997	47	22	25	95	
RASMUSSEN H (1925)	4558	2131	2427	129	35	94	35	
REESE TS (1935)	2584	634	1950	53	9	44	48	
ROSS R (1929)	4108	2642	1466	111	46	65	37	J
RUBIN H (1926)	2508	686	1822	83	27	56	30	AB
SANDBERG AA (1921)	3027	679	2348	176	28	148	17	
SIEKEVITZ P (1918)	3424	241	3183	53	9	44	64	AB
SINGER SJ (1924)	5647	2780	2867	88	13	75	64	AB
STEIN Y (1926)	2436	269	2167	88	7	81	27	
STEINER A (1936)	2885	1640	1245	54	20	34	53	
VENABLE JH (1929)	3241	2926	315	30	9	21	108	
WEISSMANN G (1930)	5372	2449	2923	154	60	94	34	
WESSELLS NK (1932)	2791	1478	1313	44	18	26	63	

Enzymology

COON MJ (1921)	2947	100	2847	79	8	71	37	
FRIDOVICH I (1929)	5141	846	4295	107	16	91	48	AB
HAYAISHI O (1920)	3437	345	3092	142	14	128	24	ABFd
KAPLAN NO (1917)	4230	251	3979	142	5	137	29	ABc

Biophysics (cont.)

	A	B	C	D	E	F	G	H
SMITH ICP (1939)	2976	273	2703	108	18	90	27	N
STOECKENIUS W (1921)	2471	479	1992	56	14	42	44	A
SUNDARALINGAM M (1931)	4022	1731	2291	111	27	84	36	
TAYLOR EW (1929)	3431	732	2699	42	7	35	81	C
TILL JE (1931)	3489	120	3369	82	8	74	42	N
TSO POP (1929)	2477	365	2112	97	10	87	25	m
URRY DW (1935)	3386	2118	1268	131	71	60	25	

Cell Biology

	A	B	C	D	E	F	G	H
ALEXANDER P (1922)	4829	1362	3467	124	45	79	38	F
BAGLIONI C (1933)	2735	556	2179	96	25	71	28	
BASERGA R (1925)	3046	855	2191	120	22	98	25	
BJORKLUND A (1945)	3549	1910	1639	105	45	60	33	
BLOBEL G (1936)	4050	2609	1441	54	16	38	75	
BORNSTEIN P (1934)	3071	1470	1601	69	23	46	44	
BRADBURY EM (1933)	2448	1337	1111	93	40	53	25	
BRANTON D (1932)	3408	1651	1757	62	10	52	54	AB
BURGER MM (1933)	4443	2450	1993	67	15	52	66	
CASPERSSON T (1910)	2599	2548	51	33	29	4	78	BCDM
COHN ZA (1926)	4162	1426	2736	75	13	62	55	A
COMMINGS DE (1935)	2662	2472	190	100	84	16	26	
DARNELL JE (1930)	7904	1731	6173	81	8	73	97	AB
DE DUVE C (1917) *	4663	1971	2692	43	12	31	108	ABFM
DEROBERTIS E (1913)	2639	1014	1625	76	19	57	34	G
FAIRBANKS G (1940)	3210	2803	407	17	4	13	188	
FARQUHAR MG (1928)	3512	1025	2487	44	8	36	79	
FRANKE WW (1940)	3031	1545	1486	127	55	72	23	
FUXE K (1938)	13319	2548	10771	238	63	175	55	I
GALLO RC (1937)	4140	1023	3117	144	42	102	28	
GREEN H (1925)	4223	739	3484	105	28	77	40	AB
GREENGARD P (1925)	8033	722	7311	119	19	100	67	AB
HARRIS H (1925)	2661	1351	1310	61	20	41	43	BC
HAYFLICK L (1928)	2824	1876	948	66	20	46	42	
HIRSCH JG (1922)	2803	484	2319	43	7	36	65	AE
HOLTZER H (1922)	3069	219	2850	80	13	67	38	
INBAR M (1939)	2577	1896	681	43	22	21	59	

Enzymology (cont.)

	A	B	C	D	E	F	G	H
KAUFMAN S (1924)	2620	820	1800	131	40	91	20	
MASSEY V (1926)	3221	1166	2055	103	19	84	31	C

Genetics

	A	B	C	D	E	F	G	H
BONNER JF (1910)	7049	1144	5905	114	12	102	61	AF
CLEAVER JE (1938)	3309	2607	702	73	48	25	45	
FREDRICKSON DS (1924)	9499	5523	3976	125	23	102	75	ABE
GILLESPIE D (1940)	2709	1896	813	50	10	40	54	
GOLDSTEIN JL (1940)	3866	1777	2089	116	47	69	33	A
HARRIS H (1919)	3729	554	3175	139	24	115	26	AC
HELINSKI DR (1933)	3039	218	2821	65	6	59	46	A
HIRSCHHORN K (1926)	3070	334	2736	189	21	168	16	
HSU TC (1917)	2820	855	1965	103	38	65	27	
KELLEY WN (1939)	3528	1266	2262	114	34	80	30	
KLEIN J (1936)	3677	1055	2622	146	32	114	25	
MCKUSICK VA (1921)	2716	769	1947	139	42	97	19	A
NEBERT DW (1940)	3226	1908	1318	97	36	61	33	
NIRENBERG M (1927) *	2914	364	2550	62	3	59	47	AI
OBRIEN JS (1934)	3721	1597	2124	90	30	60	41	
OHNO S (1928)	2702	1353	1349	147	79	68	18	AB
RUDDLE FH (1929)	3688	696	2992	172	24	148	21	AB
SHREFFLER DC (1933)	3659	780	2879	111	15	96	32	E
SIMINOVITCH L (1920)	2655	143	2512	69	5	64	38	CN
THOMAS CA (1927)	2707	359	2348	71	14	57	38	B
ZECH L (1923)	3363	349	3014	56	8	48	60	

Plant Sciences

	A	B	C	D	E	F	G	H
HAGEMAN RH (1917)	2687		2687	76		76	35	
IZAWA S (1926)	2454	903	1551	44	17	27	55	
MORRE DJ (1935)	3065	629	2436	122	27	95	25	
SKOOG F (1908)	2501	488	2013	77	6	71	32	AB
SPURR AR (1915)	2716	2455	261	20	4	16	135	
TOLBERT NE (1919)	2731	817	1914	96	8	88	28	

here, by their own request, have been reclassified—C. H. Li from pharmacology to biochemistry, and Fritz A. Lipmann from microbiology to biochemistry.

Table 1 lists the 267 authors in this group of life sciences disciplines. Their names are listed alphabetically below the appropriate discipline heading. The table contains information about each author's citations and number of cited papers. Taken all together, the authors in this section received more citations as secondary authors than as primary authors of journal articles. They also published more cited papers as secondary authors than as primary authors.

The citation rate given in column G is the average number of citations per cited paper. For example, K. Fuxe's 238 cited papers received 13,319 citations. The rate of citation is 55. On the other hand, G. Felsenfeld's 36 cited papers received 3,200 citations for a citation rate of 88.

Do not attribute special significance to small differences in individual citation counts. Keep in mind that even a list of 1,000 authors accounts for just .2 percent of regularly publishing scientists in the world. Therefore, while virtually all of the authors presented here are *of Nobel class*,[4] we have probably excluded from this study a large number of very important scientists.

Included in Table 1 is each author's year of birth. This group includes the oldest of the 1,000, Lipmann, Nobel laureate and professor of biochemistry, Rockefeller University. Lipmann was born in 1899, and coauthored his most-cited paper in this study when he was 72 years old.[5]

Since the publication of the first essay in this series in October 1981, I was saddened to hear of the death of Hans Krebs, who appears in this group of authors. In 1953, Krebs won the Nobel prize for his discovery of the citric acid cycle, now commonly known as the Krebs cycle.[6] Krebs was a member of the *SCI* editorial advisory board and inspired much of my work with continued encouragement over the years. We have also learned from Mrs. Zora Šormová that František Šorm, whose name appeared in Part 2A, died in November 1980. Šorm had been director of the Institute of Organic Chemistry and Biochemistry of the Czechoslovakian Academy of Sciences, of which he formerly served as president. He served on the editorial board of *Current Abstracts of Chemistry and Index Chemicus®*. These great scientists and humanitarians are mourned by their colleagues throughout the world.

The asterisks in Table 1 identify Nobel laureates. Along with Lipmann and Krebs, there are 13 other Nobelists in this group. Six are from molecular biology, five from biochemistry, two from cell biology, and one each from biophysics and genetics.

Fewer than half (130) of the scientists listed here are members of national academies. (The letters in column H of Table 1 denote academy memberships, while Table 2 provides a key to these letter codes.) Although the majority of scientists listed under molecular biology, biophysics, enzymology, and genetics *are* academy members, less than half of the biochemists, cell biologists, and plant scientists hold academy memberships. Thirteen authors in this group are members of more than four academies. They are listed in Table 3.

As this essay went to press, the US National Academy of Sciences (NAS) admitted 60 new members and 12 foreign associates to the academy. Of these, 17 appear in the list of 1,000 authors in Part 1. This raises the number of NAS members in this study to 257. The 17 new members will be identified in the next essay in this series.

Table 4 provides citation and authorship data for the disciplines in this

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Table 2: The academies of the 1,000 authors.

A = National Academy of Sciences, US
B = American Academy of Arts and Sciences
C = Royal Society of London, UK
D = American Philosophical Society
E = Institute of Medicine, US
F = Deutsche Akademie der Naturforscher Leopoldina, DDR
G = National Academy of Sciences of Argentina
H = Australian Academy of Science
I = Austrian Academy of Sciences
J = Royal Academy of Sciences, Letters and Fine Arts of Belgium
K = Brazilian Academy of Sciences
L = Bulgarian Academy of Science
M = More than four academy memberships
N = Royal Society of Canada
O = Academy of Sciences of Chile
P = Czechoslovakian Academy of Sciences
R = Royal Danish Academy of Sciences and Letters
S = Royal Society of Edinburgh, UK
T = Academy of Finland
U = Academy of Sciences of France
V = Académie Française
W = Bavarian Academy of Sciences, FRG
X = Göttingen Academy, FRG
Y = Indian Academy of Sciences, Bangalore
Z = Indian National Science Academy, New Delhi
a = Royal Irish Academy
b = Israel Academy of Sciences and Humanities
c = Lincei National Academy, Italy
d = Japan Academy
e = National Academy of Sciences of Mexico
f = Royal Netherlands Academy of Sciences and Letters
g = Norwegian Academy of Science and Letters
h = Polish Academy of Sciences
i = Pontifical Academy of Sciences
j = Lisbon Academy of Sciences, Portugal
k = Royal Spanish Academy
l = Royal Swedish Academy of Sciences
m = Academia Sinica, Taiwan
n = Slovene Academy of Arts and Sciences, Yugoslavia
o = Serbian Academy of Sciences and Arts, Yugoslavia
p = Hungarian Academy of Sciences
q = Academy of Sciences of Venezuela
r = Academy of Sciences of the USSR
s = Academy of the Socialist Republic of Romania
t = Heidelberg Academy of Sciences, FRG
u = Yugoslav Academy of Sciences and Arts

Table 3: Authors listed in Table 1 who are members of more than four academies.

Anfinsen C B (5) ABDRb
Brenner S (5) ABCDF
Casperson T (11) BCDFJKTYhlq
Chance B (7) ABDFGWl
de Duve C (6) ABFJUi
Jacob F (7) ABCDERU
Katchalski-Katzir E (6) ABCDFb
Katz B (5) ABCRc
Krebs H A (7) ABCDFWX
Lipmann F (5) ACDFR
Ochoa S (10) ABCDFZhikr
Palade G E (7) ABEFJis
Perutz M F (9) ABCDFISUf

group. The numbers are averages for the authors in each discipline. Biochemistry is most heavily represented here, with 85 authors on the list. Al Tappel, University of California, Davis, notes that the biochemists listed here represent a broad spectrum of interests, rather than just a few "hot spots."[7] Enzymology and plant sciences have just six authors each on the list.

The cell biologists average more citations per author, 4,175, than any other discipline in this group. Only the biochemists also averaged more than 4,000. The six enzymologists averaged more cited papers, 117, than authors in the other disciplines. The plant scientists are the oldest authors here with an average age of 62 years. When we performed our studies of botany journals,[8,9] plant biologist Jacob Levitt, Carnegie Institution, Stanford, California, provided some of the reasons plant scientists will cite biochemical papers, and why the reverse may not be true.[10] It is interesting that citation practices are reflected in the way plant scientists are represented in the academies. Incidentally, a group of six histologists, to be covered in the next part, conceivably might have been grouped with the cell biologists.

Our look at the physical and chemical scientists contained a brief discussion of some of the difficulties in assigning credit for citations to multiauthored works. In a letter to *Science*,[11] Derek J. de Solla Price argues that it is absurd to give all authors on a large team equal credit to that received by someone who is sole author of a highly cited paper. Price has proposed that such credit

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Table 4: Discipline averages for authors in life sciences (first group). A = number of authors on list. B = average number of citations received. C = average primary citations. D = average secondary citations. E = average number of cited papers. F = average papers as first author. G = average papers as secondary author. H = number of authors with academy memberships. I = number of Nobelists. J = average birth year.

Discipline	A	B	C	D	E	F	G	H	I	J
Biochemistry	85	4002	1109	2893	108	22	86	38	5	1925
Biophysics	26	3204	1069	2135	84	24	60	14	1	1927
Cell Biology	56	4175	1429	2746	95	26	69	25	2	1928
Enzymology	6	3599	588	3011	117	17	100	4	0	1923
Genetics	21	3697	1216	2481	107	26	81	12	1	1929
Molecular Biology	67	3600	1019	2581	79	15	64	37	6	1928
Plant Sciences	6	2692	882	1810	72	10	62	1	0	1920

should perhaps be assigned proportionately to each author. For example, a citation to a paper with two authors means that each author receives credit for half of a citation. Three authors of a paper would receive one-third of all citations to that paper.

Using Price's method for assigning credit to multiauthored work, we found that one-quarter of the physicists on our list of 1,000 would drop off. Applying Price's method to the present group of authors, we found that 32 would not have made the list: 15 from biochemistry, five from cell biology, four each from molecular biology and genetics, and two each from biophysics and plant sciences. Interestingly enough, all of the enzymologists would remain on the list if Price's method were applied.

T. C. Hsu, Texas Medical Center, Houston, acknowledges that Price's method of assigning credit is an "improvement," but asserts that it is still unfair to whoever did the really important work.[12] Hsu recalls a case, a common one, in which a geneticist wanted to study the chromosomes of a hospital patient who died. A pathologist, as the only person authorized to perform autopsies, provided the geneticist with a tissue sample from the patient. In the resulting paper, the pathologist appeared as coauthor, although he contributed nothing to the research. Why, wonders Hsu, should the pathologist receive equal credit with the geneticist? Why indeed! It seems to me that many scientists are unwilling to deal strongly with the ethical issues of authorship.[13]

Hsu suggests that credit should perhaps be distributed like prize money in a golf tournament. The tournament winner gets the full amount of the prize, the runner-up gets half that amount, and the third-place golfer gets half the amount of the second. Similarly, the first author of a paper would get full credit for each citation, the second author would get credit for half a citation, and the third author would get credit for one-fourth. However, Hsu doesn't believe that any credit should be assigned beyond the fourth author. He again cites the case of the geneticist and the pathologist above, noting that by the time the paper was published, "the pediatricians, the endocrinologist, and other clinicians also got their names in a 7- or 8-authored article."[12]

The point of Hsu's anecdote is well taken, but there is as yet no statistical evidence that such cases affect the ranking of scientists who regularly publish work of high impact. The unnamed pathologist may be one of thousands who publish but rarely and need an occasional paper to bolster their careers at a local level. Of greater concern are those in positions of power who abuse that power to gain greater visibility. Journals should require that authors

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sign a statement not unlike those found in patent applications. This would cover the exact nature of the contribution made by each author.

But while some abuse their power, I believe they represent a small percentage of the scientists we have identified as prolific. Those who consistently publish work of high caliber often place themselves at the end of the by-line. It would be absurd to conclude that they contributed the least. Until professional societies establish clear and unambiguous guidelines on these matters, we cannot criticize individuals for breaking unwritten laws.

* * * * *

My thanks to Thomas Di Julia and Patricia Heller for their help in the preparation of this essay.

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Current Comments®

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The 1,000 Most-Cited Contemporary Authors. Part 2C. Details on Authors in Hematology, Histology, Immunology, Microbiology, Physiology, and Virology

Number 22 May 31, 1982

This essay is the fourth part of our study of the 1,000 most-cited contemporary scientists. The study is based on data for articles published by these authors from 1965 to 1978—that's why we call them "contemporary." Citation data on these articles were taken from journals indexed in *Science Citation Index*® (*SCI*®). We did *not* count citations to books. The citation counts reflect "all-author" data—that is, each author was treated as a first author, whether or not his or her name appeared first in an article's by-line.

In Part 1 of this series, we provided the entire list of 1,000 authors.[1] That was followed by Part 2A which presented citation and authorship data on 214 scientists in the physical and chemical sciences: aeronomy, astronomy, astrophysics, geophysics, physics, chemical physics, physical chemistry, inorganic chemistry, organic chemistry, organometallic chemistry, analytical chemistry, and theoretical chemistry.[2]

Part 2B covered 267 authors in seven life sciences fields: biochemistry, biophysics, cell biology, enzymology, genetics, molecular biology, and plant sciences.[3] This part covers 238 authors in six additional life sciences that are more clinically oriented. The next part will cover 13 even more clinically oriented fields listed at the end of this essay.

As you can see, modern science is a very specialized enterprise. This is especially true of the life sciences—a total of 26 life sciences disciplines will be covered in this study. In order to avoid mistaken classifications, we decided to have the authors in this study tell us under what disciplines they should be placed. We did this by providing a questionnaire which included a list of specialties which each author simply checked off. However, some of them checked as many as five separate specialties. In these cases, we examined their papers, institutional affiliations, academy memberships, and other indicators to decide on the most appropriate "pigeonhole."

Table 1 lists the names of the 238 authors who were classified as immunologists, virologists, microbiologists, physiologists, histologists, or hematologists. The names are arranged in alphabetical order under the appropriate specialty headings in order to avoid spurious comparisons between individual citation counts. Remember that 1,000 authors represent only .2 percent of an estimated 500,000 scientists publishing worldwide. Although almost all of the 1,000 most-cited authors are *of Nobel class*,[4] many more authors not shown on these lists are equally eminent. A world academy of sciences would probably accommodate from 2,000 to 5,000 members.

Table 1 provides counts of citations and *cited* papers for each author. These totals are "disaggregated" to show the number of citations received, and the

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Table 1: The most-cited scientists in the preclinical basic sciences (second group), listed alphabetically by fields. Date of birth is in parentheses. A=total citations. B=first author citations. C=citations as a secondary author. D=total number of cited papers. E=first author papers. F=secondary-authored papers. G=citations/paper. Academy memberships are indicated by a code in column H. A key to these codes appears in Table 2. Asterisks indicate Nobel prizewinners.

Immunology	A	B	C	D	E	F	G	H
ALLISON AC (1925)	6846	3456	3390	193	75	118	35	
ALPER CA (1931)	2918	1670	1248	88	39	49	33	
AMOS DB (1923)	295[illegible]	886	2072	122	28	94	24	
AOKI T (1930)	2854	1037	1812	58	27	31	49	
ASKONAS BA (1923)	2542	920	1622	55	22	33	46	C
ASOFSKY R (1933)	2598	64	2534	81	5	76	32	
AUSTEN KF (1928)	8538	368	8170	271	13	258	31	AB
AVRAMEAS S (1930)	4717	3017	1700	113	30	83	41	
BACH FH (1934)	4975	2076	2899	154	44	110	32	
BACH JF (1940)	2806	2149	657	139	73	66	20	U
BASTEN A (1939)	2790	1580	1210	41	15	26	68	
BENACERRAF B (1920) *	8694	1071	7623	242	25	217	35	AB
BIANCO C (1941)	2644	1197	1447	28	11	17	94	
BLANDEN RV (1938)	2463	1158	1305	51	20	31	48	H
BLOCH KJ (1929)	2931	578	2353	116	15	101	25	
BORSOS T (1927)	3404	866	2538	124	15	109	27	
BOYSE EA (1923)	10159	1563	8596	174	22	152	58	ABC
BRUNNER KT (1918)	2917	1032	1885	44	7	37	66	
CANTOR H (1942)	2508	1922	586	47	24	23	53	
CEROTTINI JC (1938)	3103	1226	1877	64	25	39	48	
CLAMAN HN (1930)	3076	1597	1479	83	32	51	37	
COCHRANE CG (1930)	3848	1348	2500	89	21	68	43	
COHN M (1922)	2749	239	2510	72	23	49	38	
COOPER MD (1933)	5774	2133	3641	162	43	119	35	
DAUSSET J (1916) *	2861	1044	1817	203	69	134	14	AU
DAVID JR (1930)	4243	1544	2699	102	26	76	41	B
DIXON FJ (1920)	7456	623	6833	161	16	145	46	A
Immunology (cont.								
KLEBANOFF SJ (1927)	2476	1666	810	56	22	34	44	
KLEIN E (1925)	3910	579	3331	172	22	150	22	
KLEIN G (1925)	10347	3360	6987	387	111	276	26	ABDI
KUNKEL HG (1916)	9134	556	8578	168	17	151	54	ABEI
LAURELL CB (1919)	4283	3417	866	63	33	30	67	FI
LEDERER E (1908)	2670	187	2483	131	9	122	20	FITa
LEVINE L (1924)	4223	727	3496	195	45	150	21	
LICHTENSTEIN LM (1934)	3955	1483	2472	166	44	122	23	
MACKANESS GB (1922)	3078	1599	1479	49	17	32	62	BC
MACKAY IR (1922)	3330	803	2527	148	29	119	22	
MARCHALONIS JJ (1940)	2733	1876	857	77	32	45	35	
MCDEVITT HO (1930)	4366	1668	2698	97	20	77	45	A
MERGENHAGEN SE (1930)	3188	298	2890	95	12	83	33	
MERRILL JP (1917)	5229	252	4977	203	20	183	25	B
MICKEY MR (1923)	3127	25	3102	93	3	90	33	
MILLER JFAP (1931)	6562	2802	3760	100	41	59	65	ACH
MILSTEIN C (1927)	3130	1315	1815	87	24	63	35	AC
MOLLER G (1936)	5315	1937	3378	126	49	77	42	
MULLER EBERHARD HJ (1927)	7579	1260	6319	157	13	144	48	A
NAJARIAN JS (1927)	3446	444	3002	301	21	280	11	
NATVIG JB (1934)	2726	587	2139	136	33	103	20	
NOSSAL GJV (1931)	3409	1712	1697	79	41	38	43	ABHZ
NUSSENZWEIG V (1928)	3957	245	3712	66	11	55	59	
OPPENHEIM JJ (1934)	3631	1535	2096	87	27	60	41	
PARKER CW (1930)	3896	803	3093	162	32	130	24	
PARKS WP (1941)	2568	940	1628	71	25	46	36	
PAUL WE (1936)	6082	685	5397	145	25	120	41	A

417

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Immunology (cont.)	A	B	C	D	E	F	G	H
DOUGLAS SD (1939)	3098	993	2105	132	46	86	23	
DRAY S (1920)	2764	53	2711	131	5	126	21	
DUTTON RW (1930)	3568	976	2592	61	19	42	58	
EDELMAN GM (1929) *	7975	2566	5409	139	24	115	57	AB
FAHEY JL (1924)	4283	2006	2277	98	20	78	43	
FEFER A (1938)	2775	686	2089	97	23	74	28	
FELDMANN M (1944)	3750	2237	1513	92	42	50	40	
FRANK MM (1937)	2698	426	2272	113	23	90	23	
FRANKLIN EC (1928)	2854	365	2489	136	36	100	20	A
FREEDMAN SO (1928)	3148	58	3090	49	10	39	64	[illegible]
FUDENBERG HH (1928)	9602	516	9086	371	26	345	25	
GERSHON RK (1932)	2746	1685	1061	91	38	53	30	A
GEWURZ H (1936)	2930	879	2051	145	31	114	20	
GILDEN RV (1935)	4391	613	3778	157	21	136	27	
GOOD RA (1922)	17679	349	17330	672	27	645	26	A
GRAHAM RC (1934)	3322	2852	470	49	20	29	67	
GREAVES MF (1941)	3657	1386	2271	80	24	56	45	
GREEN I (1926)	4329	635	3694	118	18	100	36	
HABER E (1932)	5650	1538	4112	165	24	141	34	B
HELLSTROM I (1932)	6500	4279	2221	102	49	53	63	
HELLSTROM KE (1934)	6145	684	5461	103	21	82	59	
HENNEY CS (1941)	2732	1393	1339	86	39	47	31	
HEREMANS JF (1927)	3524	69	3455	113	5	108	31	FJ
HERSH EM (1935)	4324	1802	2522	159	43	116	27	
HERZENBERG LA (1931)	3569	485	3084	78	11	67	45	A
HOLBOROW EJ (1918)	3314	213	3101	101	17	84	32	
HONG R (1929)	2661	355	2306	102	18	84	26	
ISHIZAKA K (1925)	5102	2329	2773	170	45	125	30	
ISHIZAKA T (1926)	3508	1176	2332	93	38	55	37	
JOHANSSON SGO (1938)	3855	1308	2547	95	16	79	40	
JONDAL MB (1943)	3533	2270	1263	39	17	22	90	
KABAT EA (1914)	3020	523	2497	100	24	76	30	AB
KATZ DH (1943)	3544	2123	1421	92	40	52	38	

Immunology (cont.)	A	B	C	D	E	F	G	H
PERLMANN P (1919)	4373	1155	3218	127	24	103	34	
PHILLIPS RA (1937)	2487	436	2051	82	18	64	30	
PRESSMAN D (1916)	3184	28	3156	216	3	213	14	
QUIE PG (1925)	3877	772	3105	130	18	112	29	
RAPP HJ (1923)	4397	240	4157	129	10	119	34	
REISFELD RA (1926)	3680	791	2889	121	18	103	30	
ROITT IM (1927)	4014	525	3489	99	9	90	40	
ROSEN FS (1930)	4939	541	4398	147	20	127	33	
ROSENTHAL AS (1939)	3938	889	3049	80	20	60	49	
ROWE DS (1925)	2692	1407	1285	89	32	57	30	
SCHUR PH (1933)	3561	680	2881	111	22	89	32	
SCHWARTZ RS (1928)	2840	946	1894	67	19	48	42	
SEEGMILLER JE (1920)	4753	704	4049	164	15	149	28	AE
SELA M (1924)	4785	537	4248	215	13	202	22	AB[illegible]
SELL S (1935)	2719	1764	955	104	57	47	26	
SHEVACH EM (1943)	2456	1047	1409	64	15	49	38	
SIMMONS RL (1934)	4255	1454	2801	295	62	233	14	
SPRENT J (1941)	3045	890	2155	61	30	31	49	
STARZL TE (1926)	5119	2258	2861	188	66	122	27	B
STEINBERG AD (1940)	2573	900	1673	140	36	104	18	
STORB RF (1935)	3563	1223	2340	160	54	106	22	
STROBER W (1937)	2821	362	2459	83	12	71	33	
SVEJGAARD A (1937)	2439	457	1982	102	21	81	23	R
TALAL N (1934)	3550	795	2755	138	33	105	25	
TAN EM (1926)	2783	1047	1736	94	27	67	29	
TERASAKI PI (1929)	7379	734	6645	237	20	217	31	
TERRY WD (1933)	2817	609	2208	85	19	66	33	
TOMASI TB (1927)	3315	1313	2002	102	17	85	32	
UHR JW (1927)	3925	517	3408	110	10	100	35	
UNANUE ER (1934)	5100	2178	2922	119	44	75	42	
VANROOD JJ (1926)	2582	763	1819	119	37	82	21	I
WAKSMAN BH (1919)	3405	293	3112	128	23	105	26	B
WALDMANN TA (1930)	4094	1717	2377	115	29	86	35	

418

Immunology (cont.)	A	B	C	D	E	F	G	H
WARNER NL (1939)	3436	686	2750	100	28	72	34	
WARREN KS (1929)	2447	827	1620	178	60	118	13	E
WEIGLE WO (1927)	2723	513	2210	104	25	79	26	
WIGZELL HLR (1938)	7243	828	6415	136	16	120	53	
WILLIAMS RC (1928)	4037	882	3155	234	30	204	17	
WOLFF SM (1930)	2981	307	2674	149	13	136	20	
YUNIS EJ (1929)	3511	942	2569	189	35	154	18	
ZIFF M (1913)	2742	195	2547	159	10	149	17	

Virology	A	B	C	D	E	F	G	H
BALTIMORE D (1938) *	6773	2332	4441	131	21	110	51	AB
BISHOP JM (1936)	3545	423	3122	115	24	91	30	A
BLACK PH (1930)	2690	392	2298	97	14	83	27	
CHANOCK RM (1924)	6089	456	5633	200	8	192	30	A
CHOPPIN PW (1929)	3008	230	2778	64	7	57	47	A
DUESBERG PH (1936)	3071	1869	1202	50	21	29	61	
FRIEDMAN RM (1932)	2484	1115	1369	94	47	47	26	
HARTLEY JW (1928)	4208	1388	2820	38	8	30	110	
HENLE G (1912)	6816	2212	4604	103	13	90	66	A
HENLE W (1910)	6223	1423	4800	113	24	89	55	A
HILLEMAN MR (1919)	3447	530	2917	136	26	110	25	B
JOKLIK WK (1926)	3084	678	2406	64	11	53	48	A
KRUGMAN S (1911)	2911	1754	1157	63	36	27	46	AB
MELNICK JL (1914)	6427	440	5987	297	49	248	21	
MERIGAN TC (1934)	3544	677	2867	138	26	112	25	
NAHMIAS AJ (1930)	3198	1752	1446	97	37	60	32	
PRINCE AM (1928)	3754	2350	1404	94	34	60	39	
PURCELL RH (1935)	4406	880	3526	126	17	109	34	
RAPP F (1929)	3903	840	3063	196	40	156	19	
RAWLS WE (1933)	3112	1226	1886	118	29	89	26	
ROIZMAN B (1929)	3290	300	2990	88	13	75	37	A
ROWE WP (1926)	6655	1562	5093	107	21	86	62	A

Physiology (cont.)	A	B	C	D	E	F	G	H
BUTCHER RW (1930)	6511	1935	4576	46	12	34	141	
CAHILL GF (1927)	3783	943	2840	90	26	64	42	
CARLSON LA (1928)	3374	1858	1516	143	80	63	23	
COBURN JW (1932)	2928	574	2354	147	36	111	19	
COVELL JW (1936)	4579	427	4152	88	9	79	52	
DAVIS JO (1916)	2483	592	1891	108	18	90	22	AD
DIAMOND JM (1937)	2845	1643	1202	87	45	42	32	AB
DOWLING JE (1935)	2695	1536	1159	52	25	27	51	AB
GIBSON QH (1918)	2568	600	1988	110	24	86	23	ABC
GROSSMAN MI (1919)	6693	913	5780	247	51	196	27	B
HUBEL DH (1926) *	3764	2814	950	33	23	10	114	ABF
JOHNSON LR (1942)	2760	1690	1070	149	63	86	18	
JOUVET M (1925)	3137	1600	1537	97	20	77	32	U
KLEEMAN CR (1923)	2552	280	2272	111	13	98	22	
KRNJEVIC K (1927)	2821	2170	651	73	48	25	38	N
LASSEN NA (1926)	2995	912	2083	130	35	95	23	
LEVER AF (1929)	3397	15	3382	131	2	129	25	S
MACKLEM PT (1931)	3247	1018	2229	94	23	71	34	
MASSRY SG (1930)	2965	1059	1906	157	40	117	18	
MILIC-EMILI J (1931)	2439	428	2011	71	8	63	34	
ORLOFF J (1921)	2531	581	1950	60	9	51	42	E
PAGE IH (1901)	2715	222	2493	96	32	64	28	ABEI
PARK CR (1916)	3877	68	3809	63	2	61	61	A
PORTER JC (1925)	2730	243	2487	92	20	72	29	
RECTOR FC (1929)	3623	691	2932	75	9	66	48	
RUDOLPH AM (1924)	2580	1055	1525	117	25	92	22	
SALTIN B (1935)	2777	512	2265	104	19	85	26	
SCHULTZ SG (1931)	3029	1366	1663	72	24	48	42	
SELDIN DW (1920)	2924	254	2670	66	9	57	44	BCE
SWAN HJC (1922)	4196	571	3625	175	10	165	23	
WALTER R (1937)	2857	802	2055	180	61	119	15	
WEIBEL ER (1929)	2958	1386	1572	87	28	59	34	A
WIESEL TN (1924) *	3634	813	2821	32	6	26	113	AB

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Virology (cont.)

	A	B	C	D	E	F	G	H
SAMBROOK J	2442	704	1738	29	7	22	84	
SCOLNICK EM (1940)	3493	1083	2410	80	22	58	43	
SHATKIN AJ (1934)	3347	876	2471	79	13	66	42	A
STEPHENSON JR (1942)	2582	1375	1207	104	44	60	24	
STROMINGER JL (1925)	6312	524	5788	209	12	197	30	ABE
VINUELA E (1937)	3471	270	3201	37	5	32	93	B
VOGT PK (1932)	3728	979	2749	68	16	52	54	A

Microbiology

	A	B	C	D	E	F	G	H
FINLAND M (1902)	3252	472	2780	121	36	85	26	AB
GIBBONS RJ (1932)	2447	1410	1037	62	24	38	39	
GORBACH SL (1934)	3460	1654	1806	127	38	89	27	
KIRBY WMM (1914)	2568	205	2363	45	11	34	57	
KOPROWSKI H (1916)	4229	444	3785	202	14	188	20	ABu
LUDERITZ O (1920)	2980	879	2101	81	11	70	36	
MCCARTHY BJ (1934)	3943	435	3508	85	12	73	46	
NATHANS D (1928)*	2781	468	2313	54	9	45	51	A
PHILIPSON L (1929)	2911	489	2422	90	12	78	32	I
RAZIN S (1929)	2451	1119	1332	92	32	60	26	
REMINGTON JS (1931)	3354	822	2532	133	30	103	25	
ROBBINS JB (1932)	2436	864	1572	75	14	61	32	
SEVER JL (1932)	3444	704	2740	184	39	145	18	
STANIER RY (1916)	2506	1023	1483	42	7	35	59	ABC
TAKEUCHI T (1923)	4480	932	3548	430	103	327	10	
UMEZAWA H (1914)	7808	1767	6041	546	54	492	14	BFdl
WEINSTEIN L (1909)	3031	526	2505	153	45	108	19	B

Physiology

	A	B	C	D	E	F	G	H
AUSTEN WG (1930)	3792	476	3316	190	24	166	19	B
BROWN JJ (1927)	3368	1927	1441	149	65	84	22	S

Histology

	A	B	C	D	E	F	G	H
DAHLSTROM A (1941)	2513	1035	1478	59	29	30	42	
FALCK B (1927)	2721	454	2267	100	19	81	27	
FAWCETT DW (1917)	2472	1355	1117	55	19	36	44	A
JONSSON G (1941)	3290	911	2379	126	53	73	26	
LEBLOND CP (1910)	4024	57	3967	76	6	70	52	BCN
ORCI L (1937)	3600	1167	2433	193	55	138	18	
OWMAN C (1939)	3448	663	2785	160	23	137	21	

Hematology

	A	B	C	D	E	F	G	H
BAEHNER RL (1934)	2645	1583	1062	102	34	68	25	
BERNARD J (1907)	2656	465	2191	273	81	192	9	UV
BEUTLER E (1928)	4483	2542	1941	249	122	127	18	AB
CLEGG JB (1936)	2992	1381	1611	88	22	66	34	
CLINE MJ (1934)	3699	1483	2216	130	46	84	28	
FINCH CA (1915)	2951	350	2601	106	15	91	27	AB
GIBLETT ER (1921)	2447	796	1651	79	17	62	30	A
HERBERT VD (1927)	4716	2069	2647	147	49	98	32	
HOFFBRAND AV (1935)	2896	824	2072	137	41	96	21	
HOLM G (1930)	3207	901	2306	82	29	53	39	
MCCULLOCH EA (1926)	3161	249	2912	81	10	71	39	N
METCALF D (1929)	5099	2085	3014	130	66	64	39	H
NATHAN DG (1929)	3269	625	2644	142	29	113	23	
NILSSON IM (1923)	2914	478	2436	193	42	151	15	
OSKI FA (1932)	2816	952	1864	165	42	123	17	
OWEN CA (1915)	2475	266	2209	190	33	157	13	
PACKHAM MA (1927)	3233	758	2475	100	22	78	32	
RATNOFF OD (1916)	2945	681	2264	127	40	87	23	A
SELIGMANN MG (1927)	4248	1230	3018	126	33	93	33	
SHULMAN NR (1925)	2890	774	2116	63	10	53	45	
WEATHERALL DJ (1933)	3232	756	2476	115	39	76	28	C
WEISS HJ (1929)	3260	2588	672	85	62	23	38	

420

Table 2: Academy memberships of the 238 authors in the life sciences, second group, including the number of authors from each academy.

A =	National Academy of Sciences, US	52
B =	American Academy of Arts and Sciences	37
C =	Royal Society of London, UK	10
D =	American Philosophical Society	2
E =	Institute of Medicine, US	7
F =	Deutsche Akademie der Naturforscher Leopoldina, DDR	5
H =	Australian Academy of Science	4
I =	Austrian Academy of Sciences	1
J =	Royal Academy of Sciences, Letters and Fine Arts of Belgium	1
N =	Royal Society of Canada	4
R =	Royal Danish Academy of Sciences and Letters	1
S =	Royal Society of Edinburgh, UK	2
T =	Academy of Finland	1
U =	Academy of Sciences of France	4
V =	Académie Française	1
Z =	Indian National Science Academy, New Delhi	1
a =	Royal Irish Academy	1
b =	Israel Academy of Sciences and Humanities	1
d =	Japan Academy	1
f =	Royal Netherlands Academy of Sciences and Letters	1
i =	Pontifical Academy of Sciences	1
l =	Royal Swedish Academy of Sciences	6
u =	Yugoslav Academy of Sciences and Arts	1

Table 3: Authors, listed in this essay, who are members of four academies.

Klein G	ABDl
Kunkel H G	ABEl
Lederer E	FITa
Nossal G J V	ABHZ
Page I H	ABEl
Sela M	ABbi
Umezawa H	BFdl

Table 4: Newly elected members of the US National Academy of Sciences who are on the list of 1,000 most-cited contemporary authors.

Conney A H	Pharmacology
Costa E	Neuropharmacology
Cuatrecasas P	Biochemistry
Davis J O	Physiology
Eastman D E	Physics
Gibson Q H	Physiology
Herzenberg L A	Immunology
Kochi J K	Organometallic Chemistry
McLafferty F W	Analytical Chemistry
Miller J F A P	Immunology
Pastan I H	Biochemistry
Paul W E	Immunology
Shreffler D C	Genetics
Steinberg D	Biochemistry
Sugimura T	Oncology
Thomas E D	Oncology
Weissbach H	Molecular Biology

number of cited papers published, as a first or secondary author. Also listed is the author's "citation rate"—the average number of citations per paper. Citation rates are calculated by dividing citations received (column A) by the number of cited papers (column D). Each author's year of birth is shown in parentheses.

Taken as a group, the authors in these six life sciences disciplines received more citations as secondary authors than as primary authors. This was also the case for the physical, chemical, and life sciences disciplines already covered in previous essays.[2,3] Futhermore, the authors in this study also published more cited papers as secondary authors.

Nobel laureates are indicated by asterisks in Table 1. Seven of the 238 authors in this study have won Nobel prizes—three immunologists, two physiologists, one microbiologist, and one virologist.

Academy memberships are denoted by the letters in column H of Table 1. A key to these letter codes appears in Table 2. Only 86 of the 238 authors in this group hold memberships in national academies. Of these, the majority (50)

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Table 5: Discipline averages for authors listed in this essay. A=number of authors on list. B= average number of citations received. C=average primary citations. D=average secondary citations. E=average number of cited papers. F=average papers as first author. G=average papers as secondary author. H=number of authors with academy memberships. I=number of Nobelists. J=average birth year.

Discipline	A	B	C	D	E	F	G	H	I	J
Hematology	22	3283	1083	2200	132	40	92	8	0	1926
Histology	7	3153	806	2347	110	29	81	2	0	1930
Immunology	128	4132	1126	3006	129	27	102	37	3	1930
Microbiology	17	3416	836	2580	148	29	119	7	1	1923
Physiology	35	3319	971	2348	105	27	78	17	2	1927
Virology	29	4069	1056	3013	108	22	86	15	1	1929

are members of one academy, 20 are members of two, and nine of three academies. Table 3 lists those seven scientists who are members of *four* academies. Taken as a group, the average age of academy members was 58 years, and 51 for nonmembers.

The US National Academy of Sciences (NAS) recently announced the election of 60 new members, 17 of whom appear on our list of 1,000 authors. They are shown in Table 4, including the disciplines to which they were assigned in this study.

Table 5 presents citation and authorship data averaged over the entire group of scientists within each discipline. The microbiologists have the highest average of cited papers per author (148), but they are the second smallest group of authors (17). They also are the oldest, with an average age of 59 years. Immunology is the largest group with 128 authors. It has the highest average of citations per author (4,132). Only the virologists also averaged more than 4,000 citations.

C.P. Leblond, McGill University, Montreal, Canada, was surprised to see only seven authors listed under his discipline of histology. He asserts, "More than any other group, histologists are responsible for the vast amount of new knowledge on fine structure provided by the electron microscope. It seems that the knowledge of cell organelles and tissue detail is one of the most important developments of modern science. Consciously or unconsciously, physiologists and biochemists have to refer to fine structure if they are to understand intracellular happenings."[5]

Leblond offers an explanation for the presumably small number of histologists identified in this study. He suggests that the findings of histologists are "taken for granted" soon after publication. Consequently, they quite often are not explicitly cited in later publications.[5] This is a variation on the notion of "citation obliteration," a phenomenon I have discussed *ad infinitum*. However, it would be time consuming, though not impossible, to collect evidence to support the notion that this happens more often in histology than in other fields.

It may be that the average histology paper contains fewer references than, say, the average immunology paper. This has been the case for other fields that are not well represented in this list—plant sciences, for example.[3] But a check of the *Journal Citation Reports*® (*JCR*®) volume of *SCI* shows that the average number of references per paper in the leading histology journals is not significantly different from journals in immunology.

I suspect that the problem is in field definition—histology is not a well-defined field. It is rather interesting that histochemistry has produced specialty journals, but none on histology *per se*. To make sure this was not peculiar to *SCI* or *Current Contents*® (*CC*®) coverage, we checked *Ulrich's International Periodicals Directory*.[6] It lists more than 65 journals under "cytology and

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histology," but only one has the word "histology" in its title—*Archivum Histologicum Japonicum*. I therefore presume that the kind of research Leblond has in mind is reported in journals of cell biology or histochemistry. If so, why don't the histologists in our study view themselves as cell biologists? In the third part of this study, we identified 56 cell biologists.[3] It is interesting that the NAS has a section for cell biology but not for histology. Perhaps it would have been appropriate to assign our group of histologists to that category.

When we calculate average citations per author in Table 5, you should keep in mind that we are dealing with multiauthored works. In this study, we have credited each author of a multiauthored paper with the *full* citation count. Derek J. de Solla Price, Yale University, proposed giving *proportional* credit to each author instead of equal credit.[7] Thus, if a paper was written by two authors, each would get half the citations it received; three authors would get one-third each, and so on.

When we recalculated citations to multiauthored papers using Price's method, we found that 47 scientists listed here would not have made the list of 1,000 most-cited contemporary scientists: 19 from immunology, ten from physiology, seven from hematology, six from microbiology, three from virology, and two from histology.

Of course, we couldn't accurately determine who did the most important work on a multiauthored paper unless we could ask each research team "to tell the truth"—would the "real" author please stand up! T.C. Hsu, Texas Medical Center, Houston, suggested the possibility of assigning citations the way golf tournaments award prize money.[8] That is, the first author would get full credit for all citations, the second would get half that number, the third would receive half again, and so on. But credit would be limited to the first four authors. Hsu clearly never expects to be a member of a team of high-energy physicists!

Fred Rapp, Hershey Medical Center, Pennsylvania State University, believes the golf tournament method would not work.[9] He claims that in his own field of virology, the convention for ordering names is as follows: students and postdoctorates first, the senior member of the research group last, and everyone else—technicians, support staff, and drug and tissue suppliers—are placed in between. As you would expect, Rapp follows this convention. We identified 147 of his papers as having more than one coauthor. Of these, he appears last on 107, first on 27, and in between on only 13. Also, 90 of Rapp's papers were coauthored with one other person, and he is last on 71 of these. These are all in addition to ten other papers he wrote alone.

Rapp goes on to say, "It may vary from field to field, but in immunology, virology, microbiology, or the others under review in this essay, I would suspect [this ordering sequence] to be the case. You can pick up any journal you wish, look at the names of the people in Table 1 in this essay, and you'll find that most will be last on the papers. If the key person isn't first, he or she is going to be last, but not in between."[9]

Although there are no statistical data on the frequency of this practice, it's true that a large number of "senior" authors put their names last on many papers—but not necessarily their best. Rapp notes that his name does come first on review papers.[9] In fact, this is one of the reasons we were able to use first-author data for so long to identify most eminent scientists—they almost invariably write reviews or other "summary" papers on which they choose to be the first or only author. But there were important exceptions.

Until international standards for authorship are established and accepted by most leading journals, these proposals

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will remain futile and citation data will always have to be subjected to further review by peer groups. Citation data will remain, as before, *indicators* and not precise measurements of whatever one is trying to examine—eminence, importance, impact, and so on.

The next part of this essay will present citation and authorship data on 281 authors in the following life sciences disciplines: cardiology, endocrinology, gastroenterology, nephrology, neurobiology, neurology, neuropharmacology, nuclear medicine, oncology, pathology, pharmacology, psychiatry, and surgery.

* * * * *

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Current Comments®

EUGENE GARFIELD
INSTITUTE FOR SCIENTIFIC INFORMATION®

The 1,000 Most-Cited Contemporary Authors. Part 2D. Details on Authors in Cardiology, Endocrinology, Gastroenterology, Nephrology, Neurobiology, Neurology, Neuropharmacology, Nuclear Medicine, Oncology, Pathology, Pharmacology, Psychiatry, and Surgery

Number 24 June 14, 1982

This essay is the fifth in our series on the 1,000 most-cited contemporary scientists. We call these authors "contemporary" because the study is based on data for articles they published from 1965 to 1978. We limited the study to articles published in journals indexed in *Science Citation Index*® (*SCI*®). We excluded citations to books by these authors but did include citations to all papers by them, regardless of the position of their name in an article's by-line.

The first part of this series presented the entire list of authors.[1] In Part 2A, citation and authorship data were provided on 214 scientists in the physical and chemical sciences: aeronomy, astronomy, astrophysics, geophysics, physics, chemical physics, physical chemistry, inorganic chemistry, organic chemistry, organometallic chemistry, analytical chemistry, and theoretical chemistry.[2] That was followed by Part 2B, which presented details on 267 authors in seven life sciences fields: biochemistry, molecular biology, biophysics, cell biology, enzymology, genetics, and plant sciences.[3] Part 2C covered 238 authors in six additional life sciences: immunology, virology, microbiology, physiology, histology, and hematology.[4] This essay covers 281 scientists in 13 additional fields. The next part of this series concludes the study with an analysis of the institutional affiliations of all 1,000 authors for the 38 different fields represented in the overall study.

Assigning the 1,000 individual authors to these specialties turned out to be a challenging task. To avoid mistaken classifications, we let the authors themselves tell us under what disciplines they wanted to be placed. We sent each of them a questionnaire that included a "menu" of disciplines—the authors simply checked off their preferences. But some of them checked as many as five disciplines. When this happened, we relied on various indicators to assign the author to a single specialty—such as articles, institutional affiliations, academy memberships, and the like.

I've indicated elsewhere how much effort was expended to avoid the problem of homographs. In most cases, I think we've managed to avoid mistaken identities for different authors with the same surname and initials. Unfortunately, we recently learned that one of the original 1,000 names[1] turned out to be a homograph. Usually, homographs can be differentiated by simply looking at the journals in which they publish. But in this case, two people with the same name both published in (and were cited by) biochemistry journals. Had the surname been a common one, like Jones or Smith, we would have routinely checked institutional affiliations in addition to journal data. But since the name was not a common surname, we did not suspect that it might be a homograph. After we discovered that the entry for this name represented the work of two people, we

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Table 1: The most-cited scientists in the clinical sciences, listed alphabetically by fields. Date of birth is in parentheses. A = total citations. B = first author citations. C = citations as a secondary author. D = total number of cited papers. E = first author papers. F = secondary-authored papers. G = citations/paper. Academy memberships are indicated by a code in column H. A key to these codes appears in Table 2. An asterisk indicates Nobel prizewinners.

Oncology

	A	B	C	D	E	F	G	H
AARONSON SA (1942)	6072	2507	3565	140	36	104	43	
AMES BN (1928)	4937	2010	2927	75	17	58	65	AB
BALDWIN RW (1927)	2866	2330	536	122	75	47	23	
BLUMBERG BS (1925)*	5840	2637	3203	157	44	113	37	A
BODEY GP (1934)	4580	2076	2504	217	79	138	21	
CANELLOS GP (1934)	2453	600	1853	109	30	79	22	
CARBONE PP (1931)	4962	859	4103	141	19	122	35	
CLIFFORD P (1921)	3444	176	3268	66	14	52	52	
DEVITA VT (1935)	5163	1436	3727	181	38	143	28	
DOLL R (1912)	2939	880	2059	112	46	66	26	BCg
DRUCKREY H (1904)	2597	2136	461	41	34	7	63	
DULBECCO R (1914)	2921	980	1941	50	19	31	58	ABCc
FISHER B (1918)	2763	2044	719	138	86	52	20	
FISHMAN WH (1914)	2620	1201	1419	111	33	78	23	
FREI E (1924)	3909	723	3186	165	37	128	23	
FREIREICH EJ (1927)	4735	221	4514	190	11	179	24	
GOLD P (1936)	3958	2189	1769	65	17	48	60	N
GROVER PL (1933)	3158	1236	1922	61	19	42	51	
HEIDELBERGER C (1920)	4214	568	3646	134	13	121	31	A
HELLMAN L (1921)	3112	411	2701	163	15	148	19	
HENDERSON ES (1932)	2609	503	2106	94	9	85	27	
HERBERMAN RB (1940)	5027	1137	3890	207	39	168	24	
HOLLAND JF (1925)	3743	618	3125	210	32	178	17	
HUEBNER RJ (1914)	7536	1473	6063	215	10	205	35	A
IVANKOVIC S (1931)	2485	443	2042	48	15	33	51	
KAPLAN HS (1918)	5825	1282	4543	201	45	156	28	AB
LONDON WT (1932)	2878	554	2324	119	18	101	24	
MILLER EC (1920)	3044	607	2437	97	19	78	31	A
MILLER JA (1915)	5007	1571	3436	246	69	177	20	A

Cardiology (cont.)

	A	B	C	D	E	F	G	H
JAMES TN (1925)	2539	1865	674	138	74	64	18	
KAKKAR VV (1937)	2554	1419	1135	107	41	66	23	
KANNEL WB (1923)	2518	1618	900	99	46	53	25	
LARAGH JH (1924)	5681	1001	4680	149	34	115	38	
LAU SH (1926)	2943	179	2764	104	7	97	28	
LEES RS (1934)	6410	383	6027	70	15	55	91	
LEVY RI (1937)	10784	1003	9781	166	32	134	64	
LOWN B (1921)	3434	1875	1559	145	41	104	23	
MAROKO PR (1936)	3102	1833	1269	92	24	68	33	
MASON DT (1932)	5700	1577	4123	457	53	404	12	
MORROW AG (1922)	4160	360	3800	186	19	167	22	
PARMLEY WW (1936)	3512	1009	2503	126	29	97	27	
PITT B (1932)	2626	385	2241	136	23	113	19	
ROBERTS WC (1932)	3342	1470	1872	237	80	157	14	
ROBERTSON JI (1928)	3310	2	3308	128	1	127	25	S
ROSEN KM (1937)	2838	890	1948	162	33	129	17	
ROSS J (1928)	7756	1181	6575	153	27	126	50	
SCHERLAG BJ (1932)	2571	1179	1392	92	27	65	27	
SMITH TW (1936)	4037	1798	2239	141	45	96	28	
SOBEL BE (1937)	4534	669	3865	163	25	138	27	
SONNENBLICK EH (1932)	8301	1763	6538	240	31	209	34	

Gastroenterology

	A	B	C	D	E	F	G	H
ARIAS IM (1926)	3175	616	2559	112	17	95	28	
DIETSCHY JM (1932)	2504	1556	948	69	26	43	36	

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426

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Oncology (cont.)

	A	B	C	D	E	F	G	H
MOORE GE (1920)	4009	1618	2391	227	85	142	17	
MORRIS HP (1900)	4759	26	4733	305	2	303	15	
MORTON DL (1934)	4214	1911	2303	131	29	102	32	
OLD LJ (1933)	7892	1271	6621	141	14	127	55	ABE
PITOT HC (1930)	2678	423	2255	158	22	136	16	
POTTER M (1924)	3042	927	2115	108	23	85	28	A
PREUSSMAN R (1928)	2692	296	2396	67	16	51	40	
ROSENBERG SA (1927)	2448	979	1469	63	22	41	38	
RUOSLAHTI E (1940)	2885	1218	1667	90	31	59	32	
SACHS L (1924)	7561	48	7513	173	8	165	43	b
SJOGREN HO (1935)	3467	1213	2254	58	18	40	59	
SUGIMURA T (1926)	2837	785	2052	198	39	159	14	A
SUTNICK AI (1928)	2798	713	2085	85	42	43	32	
TEMIN HM (1934) *	3570	2076	1494	67	29	38	53	ABD
THOMAS ED (1920)	4702	1095	3607	203	32	171	23	A
TODARO GJ (1937)	8933	2237	6696	151	37	114	59	
WALLACH DFH (1926)	5623	1007	4616	126	31	95	44	
YOUNG RC (1940)	2503	907	1596	152	24	128	16	
ZBAR B (1938)	3131	1275	1856	67	19	48	46	

Cardiology

	A	B	C	D	E	F	G	H
BRAUNWALD E (1929)	13483	1391	12092	342	55	287	39	ABE
BRUNNER HR (1937)	2632	1041	1591	53	18	35	49	
BURCH GE (1910)	2765	1489	1276	230	151	79	12	
COOLEY DA (1920)	2889	710	2179	207	37	170	13	
DAMATO AN (1930)	4551	816	3735	185	20	165	24	
DUSTAN HP (1920)	2499	264	2235	76	11	65	32	E
EPSTEIN SE (1935)	6243	1528	4715	259	37	222	24	
FEIGENBAUM H (1933)	3123	928	2195	116	27	89	26	
GORLIN R (1926)	5894	464	5430	208	14	194	28	
HARRISON DC (1934)	4196	360	3836	296	28	268	14	
HELFANT RH (1937)	2484	841	1643	106	30	76	23	D
HOFFMAN BF (1925)	3096	308	2788	156	15	141	19	

Gastroenterology (cont.)

	A	B	C	D	E	F	G	H
HOFMANN AF (1931)	4963	1243	3720	196	30	166	25	
ISSELBACHER KJ (1925)	4771	432	4339	193	16	177	24	ABE
LIEBER CS (1931)	5300	2002	3298	185	53	132	28	
MCGUIGAN JE (1931)	2558	1510	1048	118	49	69	21	
SHERLOCK S (1918)	5670	1008	4662	274	48	226	20	
SUMMERSKILL WH (1926)	2631	272	2359	136	14	122	19	
WALSH JH (1938)	2997	888	2109	123	27	96	24	
WILLIAMS R (1933)	5234	398	4836	262	31	231	19	

Nephrology

	A	B	C	D	E	F	G	H
MICHAEL AF (1928)	3012	643	2369	120	9	111	25	

Nuclear Medicine

	A	B	C	D	E	F	G	H
WAGNER HN (1927)	4454	403	4051	304	33	271	14	

Pathology

	A	B	C	D	E	F	G	H
BENSCH KG (1928)	2507	908	1599	77	10	67	32	
COHEN AS (1926)	2749	951	1798	136	19	117	20	
ERICSSON JL (1932)	3055	861	2194	108	33	75	28	
FARBER E (1918)	2875	511	2364	113	22	91	25	
FISHER ER (1923)	2854	1243	1611	174	89	85	16	
GREY HM (1932)	4545	1368	3177	77	23	54	59	
HOBBS JR (1929)	2708	1106	1602	124	41	83	21	
LACY PE (1924)	2703	1354	1349	74	14	60	36	
LANDON J (1931)	2712	604	2108	79	15	64	34	

427

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• Pathology (cont.)

	A	B	C	D	E	F	G	H
MARCHESI VT (1935)	3395	1274	2121	51	13	38	66	
MUSTARD JF (1927)	5442	1428	4014	202	37	165	26	N
NOVIKOFF AB (1913)	2753	1293	1460	61	28	33	45	A
POLAK JM (1939)	3158	1219	1939	117	46	71	26	
POPPER H (1903)	3367	756	2611	131	46	85	25	ABF
PORTER KA (1925)	2761	446	2315	80	7	73	34	
POSTE G (1944)	2565	1403	1162	80	47	33	32	
RUBIN E (1928)	2761	1669	1092	126	54	72	21	
SCHAFFNER F (1920)	2525	642	1883	106	35	71	23	
SPICER SS (1914)	3131	712	2419	148	23	125	21	
TRUMP BF (1932)	3581	1047	2534	178	35	143	20	
TURK JL (1930)	3243	1234	2009	128	39	89	25	
WARD PA (1934)	4044	3039	1005	132	68	64	30	
WHITE JG (1929)	4002	1892	2110	219	95	124	18	

Psychiatry

	A	B	C	D	E	F	G	H
BUNNEY WE (1930)	2743	1030	1713	89	24	65	30	
DAVIS JM (1933)	3462	547	2915	165	45	120	20	
GOODWIN FK (1936)	2793	521	2272	106	19	87	26	
MURPHY DL (1936)	2624	934	1690	130	34	96	20	
SCHILDKRAUT JJ (1934)	3026	2117	909	59	37	22	51	

Endocrinology

	A	B	C	D	E	F	G	H
ARIMURA A (1923)	8569	1534	7035	248	42	206	34	
ARNAUD CD (1929)	2554	911	1643	86	17	69	29	
AURBACH GD (1927)	5245	441	4804	120	14	106	43	
AVIOLI LV (1931)	2454	1168	1286	119	36	83	20	
BARTTER FC (1914)	2816	433	2383	154	14	140	18	A
BAULIEU EE (1926)	3573	832	2741	150	25	125	23	
BERSON SA (1918)	3946	1621	2325	39	16	23	101	AC
BESSER GM (1936)	3567	857	2710	116	34	82	30	
BIERMAN EL (1930)	2606	526	2080	92	21	71	28	
BIRNBAUMER L (1939)	3198	1092	2106	38	10	28	84	

Endocrinology (cont.)

	A	B	C	D	E	F	G	H
MIGEON CJ (1923)	3274	161	3113	134	9	125	24	
MURPHY BEP (1929)	2565	2363	202	40	25	15	64	
NISWENDER GD (1940)	3749	1690	2059	93	19	74	40	
ODELL WD (1929)	4523	1745	2778	119	27	92	38	
O'MALLEY BW (1936)	6044	1653	4391	164	36	128	36	
PEARSE AGE (1916)	6151	2628	3523	186	36	150	33	F
PORTE D (1931)	4584	2009	2575	120	25	95	38	
POTTS JT (1932)	5402	461	4941	172	10	162	31	
RAISZ LG (1925)	2569	903	1666	95	33	62	27	
REDDING TW (1933)	2497	564	1933	77	24	53	32	
REICHERT LE (1932)	2913	664	2249	79	31	48	36	
RENOLD AE (1923)	2997	136	2861	122	11	111	24	
RIVIER J (1941)	2443	270	2173	76	20	56	32	
RODBELL M (1925)	4652	1904	2748	55	20	35	84	
ROSS GT (1920)	4868	231	[illegible]537	129	15	114	37	
ROTH J (1934)	6987	991	5996	221	59	162	31	
RYAN KJ (1926)	2478	483	1995	120	22	98	20	BE
SAWYER CH (1915)	3341	168	3173	149	4	145	22	AB
SCHALCH DS (1929)	2629	825	1804	82	15	67	32	
SCHALLY AV (1926) •	15340	2691	12649	489	70	419	31	AB
SOELDNER JS (1932)	2750	493	2257	119	13	106	23	
SPELLACY WN (1934)	2679	1833	846	143	107	36	18	
STEINER DF (1930)	3421	1252	2169	92	17	75	37	AB
TASHJIAN AH (1932)	3911	1723	2188	147	36	111	26	
TURKINGTON RW (1936)	2716	2049	667	84	60	24	32	
UNGER RH (1924)	6030	2064	3966	158	38	120	38	
UTIGER RD (1931)	3304	330	2974	79	9	70	41	
VALE W (1941)	4123	888	3235	99	17	82	41	
WIDE LE (1934)	2857	1138	1719	107	15	92	26	
WILLIAMS RH (1909)	3289	265	3024	127	14	113	25	
WILSON JD (1932)	3398	757	2641	95	18	77	35	
WOLFF J (1925)	2564	989	1575	92	32	60	27	
YALOW RS (1921)	4500	1704	2796	71	21	50	63	AB
YEN SSC (1927)	4108	1914	2194	124	35	89	33	

428

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Endocrinology (cont.)

	A	B	C	D	E	F	G	H
BLOOM SR (1942)	3725	1313	2412	200	64	136	18	
BOWERS CY (1924)	3681	1027	2654	124	19	105	29	
BURGUS RC (1934)	2962	975	1987	47	12	35	63	
CATT KJ (1932)	3762	1098	2664	134	34	100	28	
CHOPRA IJ (1939)	2532	2051	481	96	49	47	26	
CONN JW (1907)	2944	918	2026	85	18	67	34	AEG
COY DH (1944)	2860	490	2370	137	29	108	20	
DAUGHADAY WH (1918)	3973	484	3489	96	17	79	41	
DICZFALUSY E (1920)	2758	278	2480	153	9	144	18	
EXTON JH (1933)	3143	1806	1337	63	23	40	49	
FELIG P (1936)	3961	2071	1890	151	54	97	26	
FISHER DA (1928)	2476	851	1625	162	53	109	15	
FORSHAM PH (1915)	3449		3449	135		135	25	
FRANTZ AG (1930)	2435	443	1992	58	7	51	42	
FRIESEN HG (1934)	2845	146	2699	109	6	103	26	BH
GORDEN P (1934)	2503	586	1917	86	26	60	29	
GORSKI J (1931)	3225	463	2762	83	17	66	38	
GRODSKY GM (1927)	2761	806	1955	84	17	67	32	
GRUMBACH MM (1925)	2867	229	2638	81	3	78	35	
GUILLEMIN R (1924)*	5884	632	5252	118	23	95	49	AB
HALL R (1931)	3132	667	2465	170	29	141	18	
INGBAR SH (1925)	2977	271	2706	125	6	119	23	
KAPLAN SL (1927)	2699	551	2148	84	18	66	32	
KASTIN AJ (1934)	6460	1917	4543	220	60	160	29	
KNOBIL E (1926)	2887	104	2783	73	5	68	39	
LIDDLE GW (1921)	4743	368	4375	100	11	89	47	A
LIPSETT MB (1921)	3512	325	3187	93	23	70	37	
LUFT R (1914)	2472	421	2051	107	17	90	23	ALI
MALAISSE WJ (1936)	2957	1609	1348	141	64	77	20	
MALAISSE-LAGAE F (1936)	2606	285	2321	88	7	81	29	
MCCANN SM (1925)	5528	466	5062	179	10	169	30	
MEITES J (1913)	5231	515	4716	180	11	169	29	
MIDGLEY AR (1933)	5874	1568	4306	94	15	79	62	

Pharmacology

	A	B	C	D	E	F	G	H
ANDEN NE (1937)	5929	5412	517	92	80	12	64	
AXELROD J (1912)*	12425	1418	11007	242	31	211	51	ABCE
BECKETT AH (1920)	2741	2184	557	203	147	56	13	
BOSMANN HB (1942)	3196	2532	664	164	97	67	19	
BREESE GR (1936)	2973	996	1977	73	25	48	40	
BRODIE BB (1909)	5668	1265	4403	110	30	80	51	AEP
BURNSTOCK G (1929)	3099	1213	1886	127	30	97	24	H
BUSCH H (1923)	4921	199	4722	273	30	243	18	
CONNEY AH (1930)	7968	3127	4841	180	26	154	44	A
CORRODI H (1929)	5010	2540	2470	55	29	26	91	
CURTIS DR (1927)	3819	3104	715	75	53	22	50	CH
DALY JW (1933)	6930	1107	5823	174	34	140	39	
DOLLERY CT (1931)	3016	792	2224	186	50	136	16	
FOUTS JR (1929)	2504	552	1952	109	11	98	22	
GARATTINI S (1928)	2833	163	2670	205	12	193	13	
GILLETTE JR (1928)	4907	818	4089	143	27	116	34	
GILMAN AG (1941)	3987	3039	948	41	12	29	97	
GOLDBERG ND (1931)	2461	605	1856	67	12	55	36	
GOLDSTEIN A (1919)	2524	1148	1376	111	45	66	22	A
HAKANSON R (1937)	2563	1132	1431	156	69	87	16	
HARDMAN JG (1933)	2973	723	2250	55	9	46	54	
IVERSEN LL (1937)	8391	3237	5154	141	58	83	59	C
JOHNSTON GA (1939)	3555	630	2925	89	33	56	39	
KAMETANI T (1917)	3741	3657	84	519	499	20	7	
KAPPAS A (1926)	2613	422	2191	145	25	120	18	
KATO R (1930)	2506	2323	183	113	82	31	22	
KOCH-WESER J (1927)	3764	1985	1779	138	53	85	27	
KOPIN IJ (1929)	6646	797	5849	230	17	213	28	
KRISHNA G (1934)	3050	1593	1457	105	24	81	29	
KUNTZMAN R (1933)	3691	812	2879	101	19	82	36	
KUO JF (1933)	3846	2482	1364	67	43	24	57	
LEFKOWITZ RJ (1943)	2991	1550	1441	99	41	58	30	
LEVIN W (1940)	4690	1015	3675	156	35	121	30	

429

Pharmacology (cont.)

	A	B	C	D	E	F	G	H
LEVY G (1928)	4266	1900	2366	252	112	140	16	E
LU AYH (1937)	3578	1619	1959	90	28	62	39	
MARTIN GR (1933)	2551	345	2206	90	21	69	28	
MONCADA S (1944)	2507	875	1632	64	17	47	39	
NEFF NH (1935)	2590	741	1849	71	13	58	36	
ORRENIUS S (1937)	2481	1140	1341	87	16	71	28	
PRESSMAN BC (1926)	2595	1236	1359	55	14	41	47	
REMMER H (1919)	3013	1195	1818	73	23	50	41	
ROBISON GA (1934)	4994	2035	2959	51	11	40	97	
ROTH RH (1939)	3276	737	2539	141	32	109	23	
RUSSELL DH (1935)	2458	1572	886	120	55	65	20	
SANDLER M (1926)	2664	789	1875	118	38	80	22	
SCHWARTZ A (1929)	4526	1091	3435	284	65	219	15	
SJOERDSMA A (1924)	4758	347	4411	94	7	87	50	
SJOQVIST F (1933)	2667	264	2403	81	11	70	32	
SNYDER SH (1938)	13149	3030	10119	277	62	215	47	AB
SPECTOR S (1923)	3702	1405	2297	120	20	100	30	
SUTHERLAND EW (1915) *	10297	1924	8373	77	9	68	133	A
UNGERSTEDT U (1942)	4992	1282	3710	75	15	60	66	
VANE JR (1927)	9971	2147	7824	173	17	156	57	CI
VESELL ES (1933)	2556	1608	948	109	50	59	23	
WEISBURGER JH (1921)	2457	602	1855	154	30	124	15	
WEISS B (1937)	2888	930	1958	62	37	25	46	
WILLIAMS RT (1909)	2790	425	2365	177	41	136	15	C

Neuropharmacology

	A	B	C	D	E	F	G	H
AGHAJANIAN GK (1932)	4344	1504	2840	104	28	76	41	
BLOOM FE (1936)	4938	1695	3243	134	36	98	36	AB
CARLSSON A (1923)	4118	2878	1240	116	66	50	35	I
COSTA E (1924)	6430	726	5704	226	24	202	28	A
GLOWINSKI J (1936)	6362	2156	4206	156	13	143	40	J

Neuropharmacology (cont.)

	A	B	C	D	E	F	G	H
HOKFELT T (1940)	8268	3212	5056	171	68	103	48	
KUHAR MJ (1944)	2550	1110	1440	72	22	50	35	
PALKOVITS M (1933)	2783	1147	1636	121	40	81	23	
PLETSCHER A (1917)	3674	620	3054	126	21	105	29	
WURTMAN RJ (1936)	6330	2132	4198	226	56	170	28	

Neurobiology

	A	B	C	D	E	F	G	H
JONES EG (1939)	2523	1645	878	77	49	28	32	
OLSON LO (1942)	2597	931	1666	71	26	45	36	

Neurology

	A	B	C	D	E	F	G	H
ALTMAN J (1925)	2592	1980	612	122	67	55	21	
BRADY RO (1923)	5086	2017	3069	144	45	99	35	AB
CHANGEUX JP (1936)	6701	1555	5146	124	22	102	54	F
COGGESHALL RE (1932)	3766	615	3151	50	29	21	75	
COWAN WM (1931)	3100	831	2269	66	10	56	46	AE
ENGEL WK (1930)	4031	1120	2911	194	41	153	20	
FIELD EJ (1915)	2670	1328	1342	165	94	71	16	
GOLDSTEIN M (1924)	2577	1054	1523	96	65	31	26	
POWELL TPS (1923)	3646	254	3392	80	6	74	45	C
SEEMAN P (1934)	3276	2256	1020	76	40	36	43	
SIESJO BK (1930)	2883	501	2382	152	21	131	18	
SUZUKI K (1932)	2842	1412	1430	130	25	105	21	
THOENEN H (1928)	4506	1578	2928	142	41	101	31	F

Surgery

	A	B	C	D	E	F	G	H
DUDRICK SJ (1935)	2535	1085	1450	87	20	67	29	

430

learned that, although both were highly cited, neither alone was cited enough to meet the threshold for inclusion in the top 1,000. While it is a somewhat belated gesture to seek out the occupant of the 1,000th chair, we did so. Thus, we've added A.G. Frantz to the list of endocrinologists. Our apologies to him, and to J. Gergely, Boston Biomedical Research Institute, and J. Gergely, L. Eotros University, Budapest, Hungary.

Table 1 lists the names of the 281 authors classified under cardiology, endocrinology, gastroenterology, nephrology, neurobiology, neurology, neuropharmacology, nuclear medicine, oncology, pathology, pharmacology, psychiatry, and surgery. We originally had a separate category for neuroendocrinology, which included one author. However, we recently learned that several authors listed as endocrinologists could just as easily have been classified as neuroendocrinologists. Unfortunately, we could not contact those individuals to confirm their reassignment before we went to press. Instead, we abolished the category to avoid giving the unfair impression that only one neuroendocrinologist appears on our list. The single author who formerly appeared in that category was transferred to endocrinology.

In order to discourage invidious comparisons between individual citation counts, the authors are arranged in alphabetical order under the appropriate specialty heading. Even 1,000 authors are a small sample of the estimated 500,000 regularly publishing scientists worldwide. If we were able to extend the list to include 2,000 authors, almost all would still be *of Nobel class*.[5] Indeed, 5,000 names would be a more appropriate number to cover science worldwide.

Table 1 includes information on each author's total citations and the total number of *cited* papers. These figures are broken down to show how many citations were received as a primary or secondary author, and how many papers were published as a first or second author. Also, the author's citation rate is listed—that is, the average number of citations per paper. Citation rate is calculated by dividing total citations received (column A) by the total number of cited papers (column D). The author's year of birth is shown in parentheses after the name.

Table 2: Academy memberships of the 281 authors in the life sciences, third group, including the number of authors from each academy.

A	=	National Academy of Sciences, US	37
B	=	American Academy of Arts and Sciences	19
C	=	Royal Society of London, UK	9
D	=	American Philosophical Society	2
E	=	Institute of Medicine, US	10
F	=	Deutsche Akademie der Naturforscher Leopoldina, DDR	4
G	=	National Academy of Sciences of Argentina	2
H	=	Australian Academy of Science	2
J	=	Royal Academy of Sciences, Letters and Fine Arts of Belgium	1
L	=	Bulgarian Academy of Science	1
N	=	Royal Society of Canada	3
P	=	Czechoslovakian Academy of Sciences	1
S	=	Royal Society of Edinburgh, UK	1
b	=	Israel Academy of Sciences and Humanities	1
c	=	Lincei National Academy, Italy	1
f	=	Royal Netherlands Academy of Sciences and Letters	1
g	=	Norwegian Academy of Science and Letters	1
l	=	Royal Swedish Academy of Sciences	2

Taken as a group, the authors in 12 of the 13 life sciences in this study received more citations as secondary authors than as primary authors. This was also observed in all the other fields covered in previous essays.[2-4] The lone exception in the present study is neurobiology. But only two authors are included here, and one of them (E.G. Jones) received nearly twice as many primary author citations as secondary author citations. If a larger group of neurobiologists had been included, I suspect that the typical pattern would apply.

Six of the 281 authors in this part are Nobel laureates. They are indicated by an asterisk in Table 1. Two endocrinologists, two pharmacologists, and two oncologists have won the Nobel prize. All six are also members of national academies. Academy memberships are denoted by letters in column H of Table 1. A

Table 3: Discipline averages for authors in this essay. A=number of authors on list. B=average number of citations received. C=average primary citations. D=average secondary citations. E=average number of cited papers. F=average papers as first author. G=average papers as secondary author. H=number of authors with academy memberships. I=number of Nobelists. J=average birth year.

Discipline	A	B	C	D	E	F	G	H	I	J
Oncology	48	4065	1155	2910	134	30	104	16	2	1926
Cardiology	33	4439	1018	3421	167	35	133	4	—	1930
Gastroenterology	10	3980	992	2988	167	31	136	1	—	1929
Nephrology	1	3012	643	2369	120	9	111	—	—	1928
Nuclear Medicine	1	4454	403	4051	304	33	271	—	—	1927
Pathology	23	3193	1172	2021	118	36	82	3	—	1927
Psychiatry	5	2930	1030	1900	110	32	78	—	—	1934
Endocrinology	77	3818	975	2843	120	25	95	13	2	1928
Neuropharmacology	10	4980	1718	3262	145	37	108	4	—	1932
Pharmacology	57	4275	1470	2804	134	44	90	12	2	1930
Neurobiology	2	2560	1288	1272	74	38	36	—	—	1940
Surgery	1	2535	1085	1450	87	20	67	—	—	1935
Neurology	13	3667	1269	2398	119	39	80	5	—	1928

key to these letter codes appears in Table 2. Only 58 authors are academy members. This includes four authors on the list elected to the US National Academy of Sciences after the list was prepared: T. Sugimura and E.D. Thomas, oncology; E. Costa, neuropharmacology; and A.H. Conney, pharmacology. Taken as a group, the average age of academy members is 59 years, and 52 years for nonmembers. Of the 58 academy members listed here, 31 are members of one academy, 16 of two, and nine of three academies. Both R. Dulbecco, oncology, and J. Axelrod, pharmacology, are members of four national academies.

Richard Wurtman, Massachusetts Institute of Technology, informed us that he and several other authors on the list worked with Axelrod at the National Institute of Mental Health (NIMH) from 1963 to 1965.[6] S. Snyder, J. Glowinski, L. Iversen, and Wurtman were fellows in Axelrod's laboratory of clinical science, section on pharmacology.[6] It's an interesting coincidence that this study is based on citation data for articles published by these authors since they left Axelrod's lab at NIMH. This "invisible college" of Axelrod's fellows has done quite well since then. It would be interesting to "map" patterns of citations between former research collaborators. We might see these invisible colleges continuing over time, even though the collaborators are no longer at the same laboratory or institution.

Table 3 presents average citation and authorship data for the entire group of scientists under each discipline heading. However, data presented for nephrology, nuclear medicine, and surgery are absolute numbers, since each of these disciplines is represented by only a single author. The cardiologists and gastroenterologists have the highest average of cited papers per author, 167, while the neuropharmacologists have the highest average of citations per author, 4,980. The oncologists, cardiologists, and pharmacologists also averaged more than 4,000 citations. The oncologists are the oldest authors, with an average age of 56 years. The two neurobiologists are the youngest, averaging 42 years.

When we calculate average citations per author, you should keep in mind that we are dealing with multiauthored works. In this study, we have credited each author of a multiauthored paper with the *full* total of citations it received. Derek J. de Solla Price, Yale University, suggested in a letter to *Science* that *proportional* credit should be given to each author instead of *equal* credit.[7] Thus, if a paper were written by two authors, each would get half the total citations it received; three authors would get one-third each, and so on.

We recalculated citations to multiauthored papers using Price's method to see what effect it would have on our group of authors. We found that 78 of the 281 scientists in the present study would not

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have made the list: 28 from endocrinology, 14 from cardiology, 12 each from oncology and pharmacology, four from pathology, and three from psychiatry. The following fields would have one less author each under Price's method of proportional citation credit: gastroenterology, nephrology, neuropharmacology, neurobiology, and surgery.

All of this indicates that we are not dealing with simple statistical problems, and that the data should be examined with extreme caution. Clearly, any of the fields that are represented by a small number of names needs to be examined more carefully. Consideration must also be given to many factors which add bias to this particular sample. When we confine citation counts to even more current literature, the average age of cited scientists *should* go down. But we've seen a number of examples where high impact appears to be age independent.

The concluding part of this study will present details on the institutional affiliations of all 1,000 most-cited contemporary authors.

* * * * *

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Current Comments®

EUGENE GARFIELD
INSTITUTE FOR SCIENTIFIC INFORMATION®

The 1,000 Most-Cited Contemporary Scientists. Part 3. Details on Their Current Institutional Affiliations

Number 27 July 5, 1982

This essay is the sixth in our series on the 1,000 contemporary authors most cited for articles they published from 1965 to 1978. The study is based on data from journals indexed in *Science Citation Index*® (*SCI*®). Citations to books were not included. The data were taken from our "all author" files, which treat all authors on every article as if they were listed first in the by-line.

Part 1 of this series described in detail how the authors were identified and presented the entire list of names.[1] Part 2A provided citation and authorship data on 214 scientists in 12 physical and chemical sciences.[2] Parts 2B, 2C, and 2D covered a total of 786 authors in 26 life sciences fields.[3-5] Table 1 lists all the fields covered in each of these four parts of this study.

This part of the series concludes our study with an analysis of the current institutional affiliations of all 1,000 most-cited contemporary scientists. We started by checking ISI®'s *Current Bibliographic Directory of the Arts & Sciences*® to get their addresses. A letter was sent to each of the authors asking them to fill out a questionnaire and provide a list of their publications. Thus, the institutions from which they wrote back to us during our study (1979-1980) were listed as their current affiliations. If we didn't get a response, we tried to contact the authors by telephone to make sure their addresses were up to date. In the event that an author was retired or deceased, we checked their last publication for the most recent affiliation. Finally, we incorporated any changes in affiliation that were brought to our attention following the publication of preceding parts of this study.

In retrospect, it may have been more interesting to list *all* the institutions which each author was affiliated with over the 14-year period covered in this study, in addition to their current affiliations. This certainly would have been a difficult task, but not an impossible one. Keep in mind that the average author in this study published 121 cited papers.[1] Thus, we would have to check over 120,000 papers in order to trace all the authors' career histories. The resulting information simply may not have been worth the time and expense such a massive effort would require. Of course, we could have asked the authors themselves to provide their institutional affiliations from 1965 to 1978. But we felt this would overburden many of the authors who were kind enough to fill out our questionnaire in the first place.

Table 2 lists the current affiliations of all 1,000 authors in this study. The list is arranged in alphabetical order by country. US institutions are further organized by state. Institutions are ranked by total number of most-cited authors un-

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Table 1: Scientific fields covered in Parts 2A, 2B, 2C, and 2D of the 1,000 most-cited contemporary authors study.

Part 2A
aeronomy
analytical chemistry
astronomy
astrophysics
chemical physics
geophysics
inorganic chemistry
organic chemistry
organometallic chemistry
physical chemistry
physics
theoretical chemistry

Part 2B
biochemistry
biophysics
cell biology
enzymology
genetics
molecular biology
plant sciences

Part 2C
hematology
histology
immunology
microbiology
physiology
virology

Part 2D
cardiology
endocrinology
gastroenterology
nephrology
neurobiology
neurology
neuropharmacology
nuclear medicine
oncology
pathology
pharmacology
psychiatry
surgery

der each country and/or state heading. Also shown are the authors affiliated with each institution and the disciplines they were assigned to in this study. Actually, the authors assigned themselves to their discipline by choosing from a "menu" of specialties on the questionnaire they received. If they checked off more than one discipline, we decided which one would be most appropriate after examining their articles, academy memberships, institutional affiliations, and so on.

Of the 252 institutions listed in Table 2, 147 are located in the US and 736 authors are affiliated with them. Thus, the US accounts for 58 percent of the institutions and 74 percent of the scientists in this study. The European countries account for 76 institutions (31 percent) and 206 authors (21 percent).

The UK has 28 institutions and 85 authors. Ten institutions are located in Switzerland, and they account for 13 authors. However, two institutions based in Geneva are actually international research organizations—CERN (European Organization for Nuclear Research), two authors, and World Health Organization, one author. Canada and the Federal Republic of Germany are represented by nine institutions each, with 23 and 21 authors, respectively. Japan accounts for eight institutions, with 11 authors. Seven French institutions with 26 authors appear on the list. Twelve authors are affiliated with six Australian institutions. Only six institutions from Sweden are shown, but they represent 42 authors—the third largest group in the study. Table 3 shows the number of institutions that are located in the 21 countries represented in this study, including the number of authors.

Just ten of the 147 US institutions account for 46 percent of US authors: the University of California's ten branch campuses (79), National Institutes of Health (79), Harvard University (43), Stanford University (35), the University of Texas' five campuses (28), University of Wisconsin (21), Yale University (15), Massachusetts Institute of Technology (14), Rockefeller University (14), and the Veteran's Administration (VA) hospital system (13). Although we list the VA as being based in Washington, DC, the VA hospitals are actually located in five states—California (3), Louisiana (3), New York (4), Texas (2), and Wisconsin (1).

Three of the UK's 28 institutions account for 53 percent of UK authors: the Universities of London (21), Cambridge (15), and Oxford (9). The Federal Republic of Germany's Max Planck Society for the Advancement of Science alone claims 43 percent of that country's

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Table 3: Locations of institutions of the 1,000 authors by country, in alphabetical order, and number of authors. A = country. B = number of institutions. C = number of authors.

A	B	C
Argentina	1	1
Australia	6	12
Belgium	2	3
Canada	9	23
Czechoslovakia	1	1
Denmark	3	4
Federal Republic of Germany	9	21
Finland	1	1
France	7	26
Hungary	1	1
Israel	4	10
Italy	3	4
Japan	8	11
The Netherlands	3	3
Norway	1	1
Spain	1	1
Sweden	6	42
Switzerland	10	13
Union of Soviet Socialist Republics	1	1
United Kingdom	28	85
United States	147	736
TOTAL	252	1,000

authors. Similarly, Sweden's Karolinska Institute alone accounts for 45 percent of Swedish authors on the list.

Table 4 shows where the 147 US institutions and 736 authors are located by state. New York accounts for the largest number of institutions (22), but it is tied with Massachusetts for third place in terms of the number of authors (85). Massachusetts and California each have 13 institutions, but California claims the largest number of authors (156). Twelve institutions and 29 authors are based in Pennsylvania. New Jersey follows with ten institutions and 30 authors. Maryland's seven institutions account for the second largest group of authors by state (96), 82 of whom work for the government—National Institutes of Health, Food and Drug Administration, Department of Agriculture, and National Aeronautics and Space Administration.

This institutional analysis is a by-product of the basic study, which was primarily intended to identify the 1,000 most-cited contemporary scientists publishing from 1965 to 1978. But it is interesting to note its similarity to a recent study by J. P. Rushton and S. Meltzer, University of Western Ontario, Canada.[6] They used the 1977 *SCI*, *Social Sciences Citation Index*®, and *Arts & Humanities Citation Index*™ to rank 169 universities in the US, UK, and Canada by the total number of publica-

Table 4: Locations of US institutions of the 736 US authors by state, in alphabetical order, and number of authors. A = state. B = number of institutions. C = number of authors.

A	B	C
Alabama	1	5
Alaska	1	1
Arizona	1	1
California	13	156
Colorado	5	12
Connecticut	2	17
Florida	2	6
Georgia	4	6
Illinois	6	25
Indiana	3	10
Iowa	1	1
Louisiana	1	4
Maryland	7	96
Massachusetts	13	85
Michigan	4	13
Minnesota	2	10
Missouri	3	13
New Jersey	10	30
New Mexico	1	2
New York	22	85
North Carolina	3	11
Ohio	4	6
Oklahoma	2	2
Oregon	1	1
Pennsylvania	12	29
South Carolina	2	3
Tennessee	2	8
Texas	6	37
Utah	2	3
Virginia	1	1
Washington, DC	5	18
Washington	3	17
Wisconsin	2	22
TOTAL	147	736

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Table 2: Institutional affiliations of the 1,000 most-cited contemporary authors, in alphabetical order by country. US institutions also are arranged by state, in alphabetical order. Institutions are arranged in order of the number of most-cited authors affiliated with them. Authors affiliated with each institution are shown, including the categories to which they were assigned. All affiliations reflect the institution from which the authors wrote to us during our study (1979-1980), unless they have notified us of a subsequent change.

Argentina (1)
University of Buenos Aires (1)
De Robertis E D P (Cell Biol.)

Australia (12)
Walter and Eliza Hall Institute, Melbourne (5)
MacKay I R (Immunology)
Metcalf D (Hematology)
Nossal G J V (Immunology)
Miller J F A P (Immunology)
Warner N L (Immunology)

Australian National University, Canberra (2)
Blanden R V (Immunology)
Curtis D R (Pharmacology)

University of Sydney (2)
Basten A (Immunology)
Johnston G A (Pharmacology)

Anglo-Australian Observatory, Epping (1)
Morton D C (Astrophysics)

Monash University, Clayton (1)
Linnane A W (Biochemistry)

University of Adelaide (1)
Bowie J H (Organic Chem.)

Belgium (3)
Free University of Brussels (2)
Malaisse W J (Endocrinology)
Malaisse-Lagae F (Endocrinology)

Canada (cont'd.)
University of Guelph (1)
Clark H C (Organomet. Chem.)

University of Manitoba, Winnipeg (1)
Friesen H G (Endocrinology)

Czechoslovakia (1)
Czechoslovak Academy of Sciences, Prague (1)
Sorm F (Organic Chem.)

Denmark (4)
University of Copenhagen (2)
Hansen O (Physics)
Saltin B (Physiology)

Bispebjerg Hospital, Copenhagen (1)
Lassen N A (Physiology)

University Hospital, Copenhagen (1)
Svejgaard A (Immunology)

Federal Republic of Germany (21)
Max Planck Society for the Advancement of Science, Munich (9)
Max Planck Institut fur Immunbiologie, Freiburg
Druckrey H (Oncology)
Luderitz O (Microbiol.)
Westphal O H (Biochemistry)

Max Planck Institut fur Biophysikalische Chemie, Gottingen
Weber K (Biochemistry)
Osborn M (Cell Biol.)

Finland (1)
University of Oulu (1)
Kivirikko K I (Biochemistry)

France (26)
CNRS (7)
Centre de Neurochimie, Strasbourg
Mandel P (Cell Biol.)

Institut de Chimie des Substances Naturelles, Gif-sur-Yvette
Barton D H R (Organic Chem.)

Institut de Recherche en Biologie Moleculaire, Paris
Bernardi G (Molec. Biol.)

Institut de Recherches Scientifiques sur le Cancer, Villejuif
Bernhard W (Biophysics)

Laboratoire d'Annecy-le-Vieux de Physique des Particules
Vannucci F (Physics)

Laboratoire de Biochimie, Gif-sur-Yvette
Lederer E (Biochemistry)

Laboratoire de l'Accelerateur Linaire, Orsay
Jean-Marie B (Physics)

Institut Pasteur, Paris (6)
Avrameas S (Immunology)
Changeux J P (Neurology)
Gros F (Molec. Biol.)

Israel (10)
Weizmann Institute of Science, Rehovot (6)
Harari H (Physics)
Inbar M (Cell Biol.)
Katchalski-Katzir E (Biophysics)
Sachs L (Oncology)
Sela M (Immunology)
Sharon N (Biochemistry)

Hebrew University of Jerusalem (2)
Levine R D (Theoretical Chem.)
Razin S (Microbiol.)

Hadassah University Hospital, Jerusalem (1)
Stein Y (Cell Biol.)

Tel-Aviv University (1)
Jortner J (Physics)

Italy (4)
University of Rome (2)
Antonini E (Biochemistry)
Wyman J (Molec. Biol.)

Mario Negri Institute, Milan (1)
Garattini S (Pharmacology)

University of Bologna (1)
Giacomelli G M (Physics)

Japan (11)
Institute of Microbial Chemistry, Tokyo (2)
Takeuchi T (Microbiol.)
Umezawa H (Microbiol.)

60-347 0 - 86 - 15

Belgium (cont'd.)
Catholic University of Louvain (1)
Heremans J F (Immunology)

Canada (23)
McGill University, Montreal (7)
Freedman S O (Immunology)
Gold P (Oncology)
Krnjevic K (Physiology)
Leblond C P (Histology)
MacKlem P T (Physiology)
Milic-Emili J (Physiology)
Murphy B E P (Endocrinology)

University of Toronto (5)
Eylar E H (Biochemistry)
Farber E (Pathology)
Packham M A (Hematology)
Polanyi J C (Physical Chem.)
Seeman P (Neurology)

Ontario Cancer Institute, Toronto (3)
McCulloch E A (Hematology)
Phillips R A (Immunology)
Till J E (Biophysics)

McMaster University, Hamilton (2)
Mustard J F (Pathology)
Rawls W E (Virology)

National Research Council, Ottawa (2)
Ingold K U (Organic Chem.)
Smith I C P (Biophysics)

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Siminovitch L (Genetics)

University of Alberta (1)
Henderson J F (Biochemistry)

FRG (cont'd.)
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Max Planck Institut fur Biologie, Tubingen
Klein J (Genetics)

Max Planck Institut fur Festkorperforschung, Stuttgart
Cardona M (Physics)

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Franke W W (Cell Biol.)
Ivankovic S (Oncology)
Preussmann R (Oncology)

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Klingenberg M E (Biophysics)

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Schmidbaur H (Organomet. Chem.)

Erlangen-Nuremberg University (1)
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Technical University of Berlin (1)
Bohlmann F (Organic Chem.)

Tubingen University (1)
Remmer H (Pharmacology)

University of Bielefeld (1)
Muller A (Inorganic Chem.)

University of Frankfurt (1)
Greiner W (Physics)

France (cont'd.)
Jacob F (Molec. Biol.)
Monod J L (Molec. Biol.)
Stanier R Y (Microbiol.)

University of Paris (6)
VII
Bernard J A (Hematology)
Dubois J E (Physical Chem.)
Seligmann M G (Hematology)

V
Bach J F (Immunology)

VI
Le Pichon X (Geophysics)

XI, Orsay
Baulieu E E (Endocrinology)

Louis Pasteur University, Strasbourg (3)
Chambon P H (Molec. Biol.)
Lehn J (Physical Chem.)
Osborn J A (Inorganic Chem.)

College of France, Paris (2)
Dausset J (Immunology)
Glowinski J (Neuropharmacol.)

Institute of Cancer & Immunogenetics, Villejuif (1)
Mathe G (Cell Biol.)

University Claude Bernard, Lyon (1)
Jouvet M (Physiology)

Hungary (1)
Semmelweis University Medical School, Budapest (1)
Palkovits M (Neuropharmacol.)

Japan (cont'd.)
Kyoto University (2)
Hayaishi O (Enzymology)
Nozaki H (Organic Chem.)

National Cancer Research Institute, Tokyo (2)
Nishimura S (Molec. Biol.)
Sugimura T (Oncology)

Hoshi College of Pharmacy, Tokyo (1)
Kametani T (Pharmacology)

Keio University, Tokyo (1)
Kato R (Pharmacology)

Osaka University (1)
Sato R (Biochemistry)

Sagami Chemical Research Center, Kanagawa (1)
Morino Y (Physical Chem.)

Shinrakuen Hospital, Nigatashi (1)
Aoki T (Immunology)

The Netherlands (3)
State University of Leiden (1)
van Rood J J (Immunology)

State University of Utrecht (1)
van Deenen L L (Biochemistry)

University of Amsterdam (1)
Borst P (Molec. Biol.)

Norway (1)
Oslo University (1)
Natvig J B (Immunology)

438

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Vinuela E (Virology)

Sweden (42)
Karolinska Institute, Stockholm (20)
Carlson L A (Physiology)
Caspersson T O (Cell Biol.)
Diczfalusy E (Endocrinology)
Fuxe K G (Cell Biol.)
Hamberg M (Biochemistry)
Hokfelt T (Neuropharmacol.)
Holm O G (Hematology)
Johansson S G O (Immunology)
Jondal M B (Immunology)
Jonsson G (Histology)
Klein E (Immunology)
Klein G (Immunology)
Luft R (Endocrinology)
Olson L O (Neurobiology)
Orrenius S (Pharmacology)
Samuelsson B (Biochemistry)
Sjoqvist F F G (Pharmacology)
Sjovall J G (Biochemistry)
Ungerstedt U (Pharmacology)
Zech L (Genetics)

University of Lund (8)
Bjorklund A (Cell Biol.)
Falck B O F (Histology)
Hakanson R (Pharmacology)
Laurell C B (Immunology)
Nilsson I M (Hematology)
Owman C S O (Histology)
Siesjo B K (Neurology)
Sjogren H O (Oncology)

Uppsala University (7)
Anden N E B (Pharmacology)
Ericsson J L E (Pathology)

Switzerland (cont'd.)
Swiss Institute for Experimental Cancer Research, Lausanne (1)
Brunner K T (Immunology)

University of Lausanne (1)
Brunner H R (Cardiology)

World Health Organization, Geneva (1)
Rowe D S (Immunology)

Zurich University (1)
Schmid H (Physical Chem.)

Union of Soviet Socialist Republics (1)
Shemyakin Institute of Bioorganic Chemistry, Moscow (1)
Ovchinnikov Y A (Biochemistry)

United Kingdom (85)
University of London (21)
University College
Burnstock G (Pharmacology)
Clark R J H (Inorganic Chem.)
Feldmann M (Immunology)
Katz B (Biophysics)
Miledi R (Biophysics)
Raff M C (Cell Biol.)

Royal Postgraduate Medical School
Bloom S R (Endocrinology)
Datta N (Molec. Biol.)
Dollery C T (Pharmacology)
Pearse A G E (Endocrinology)
Polak J M (Pathology)

Imperial College of Science and Technology
Blow D M (Biophysics)
Hartley B S (Molec. Biol.)
Wilkinson G (Organomet. Chem.)

UK (cont'd.)
Strangeways Research Laboratory
Kodicek E H (Biochemistry)

University of Oxford (9)
Brownlee G G (Molec. Biol.)
Clegg J B (Hematology)
Doll R (Oncology)
Harris H (Cell Biol.)
Krebs H A (Biochemistry)
Phillips D C (Molec. Biol.)
Powell T P S (Neurology)
Weatherall D J (Hematology)
Williams R J (Inorganic Chem.)

Medical Research Council (5)
Blood Pressure Unit, Glasgow
Brown J J (Physiology)
Lever A F (Physiology)
Robertson J I S (Cardiology)

National Institute for Medical Research, London
Askonas B A (Immunology)
Tata J R (Biochemistry)

Institute of Cancer Research, London (3)
Alexander P (Cell Biol.)
Grover P L (Oncology)
Sims P (Molec. Biol.)

King's College Hospital, London (3)
Clifford P (Oncology)
Kakkar V V (Cardiology)
Williams R (Gastroenterol.)

Royal Free Hospital, London (3)
Chapman D (Biophysics)
Hoffbrand A V (Hematology)
Sherlock S (Gastroenterol.)

UK (cont'd.)
Macaulay Institute for Soil Research, Aberdeen (1)
West T S (Analyt. Chem.)

National Institute for Research in Dairying, Shinfield (1)
Andrews P (Biochemistry)

Queen Charlotte's Maternity Hospital, London (1)
Sandler M (Pharmacology)

Royal College of Surgeons of England, London (1)
Turk J L (Pathology)

Science Research Council, Rutherford Laboratory, Didcot (1)
Phillips R J (Physics)

University of Leeds (1)
Shaw B L (Inorganic Chem.)

University of Leicester (1)
Symons M C R (Inorganic Chem.)

University of Liverpool (1)
Goodwin T W (Biochemistry)

University of Manchester (1)
Haszeldine R N (Organomet. Chem.)

University of Nottingham (1)
Baldwin R W (Oncology)

Welsh National School of Medicine, Cardiff (1)
Hall R (Endocrinology)

439

Sweden (cont'd.)

Kurland C G (Molec. Biol.)
Philipson L (Microbiol.)
Porath J O (Biochemistry)
Wide L E (Endocrinology)
Wigzell H L R (Immunology)

Stockholm University (3)
Ernster L (Biochemistry)
Moller G (Immunology)
Perlmann P (Immunology)

Gothenburg University (3)
Carlsson A (Neuropharmacol.)
Corrodi H (Pharmacology)
Dahlstrom A (Histology)

Pharmacia Diagnostics, Uppsala (1)
Axen R (Biochemistry)

Switzerland (13)

CERN, Geneva (2)
Veneziano G (Physics)
Zumino B (Physics)

University of Basel (2)
Burger M M (Cell Biol.)
Heilbronner E (Organic Chem.)

University of Geneva (2)
Orci L (Histology)
Renold A E (Endocrinology)

Bern University (1)
Weibel E R (Physiology)

Kanton Hospital, Basel (1)
Pletscher A (Neuropharmacol.)

Ludwig Institute for Cancer Research,
Lausanne (1)
Cerottini J C (Immunology)

UK (cont'd.)

St. Bartholomew's Hospital Medical College
Besser G M (Endocrinology)
Landon J (Pathology)

St. Mary's Hospital Medical School
Porter K A (Pathology)
Williams R T (Pharmacology)

Chelsea College
Beckett A H (Pharmacology)

Middlesex Hospital Medical School
Roitt I M (Immunology)

Westminister Medical School
Hobbs J R (Pathology)

University of Cambridge (15)

Laboratory of Molecular Biology
Barrell B G (Molec. Biol.)
Brenner S (Molec. Biol.)
Klug A (Molec. Biol.)
Milstein C (Immunology)
Perutz M F (Molec. Biol.)
Sanger F (Molec. Biol.)

Department of Chemistry
Battersby A R (Organic Chem.)
Johnson B F G (Organic Chem.)
Lewis J (Inorganic Chem.)
Williams D H (Organic Chem.)

Department of Biochemistry
Lehmann H (Molec. Biol.)
Northcote D H (Cell Biol.)

Cavendish Laboratory
Mott N F (Physics)

Neurochemical Pharmacology Unit
Iversen L L (Pharmacology)

UK (cont'd.)

University of Sussex, Brighton (3)
Chatt J (Inorganic Chem.)
Lappert M F (Organomet. Chem.)
Mason R (Inorganic Chem.)

University of Bristol (2)
Green M (Inorganic Chem.)
Stone F G A (Organomet. Chem.)

University of Edinburgh (2)
Cowley R A (Physics)
Loening U E (Molec. Biol.)

Wellcome Research Laboratories, Kent (2)
Moncada S (Pharmacology)
Vane J R (Pharmacology)

Agricultural Research Council, Cambridge (1)
Dawson R M C (Biochemistry)

Beatson Institute for Cancer Research,
Glasgow (1)
Paul J (Molec. Biol.)

Crossley Neurological Research Centre,
Newcastle upon Tyne (1)
Field E J (Neurology)

Glynn Research Laboratories, Cornwall (1)
Mitchell P D (Biochemistry)

Imperial Cancer Research Fund, London (1)
Greaves M F (Immunology)

London Hospital (1)
Holborow E J (Immunology)

United States (736)

Alabama (5)

University of Alabama, Birmingham (5)
Cooper M D (Immunology)
Dustan H P (Cardiology)
James T N (Cardiology)
Miller E J (Biochemistry)
Urry D W (Biophysics)

Alaska (1)

University of Alaska, Fairbanks (1)
Akasofu S I (Aeronomy)

Arizona (1)

University of Arizona, Tucson (1)
Russell D H (Pharmacology)

California (156)

University of California (79)

Berkeley

Abrams G S (Physics)
Ames B N (Oncology)
Casida J E (Biochemistry)
Chinowsky W (Physics)
Clark A J (Molec. Biol.)
Cohen M L (Physics)
Duesberg P H (Virology)
Goldhaber G (Physics)
Kadyk J A (Physics)
Koshland D E (Biochemistry)
Miller W H (Physics)
Moore C B (Physics)
Muetterties E L (Inorganic Chem.)
Packer L (Biophysics)
Rubin H (Cell Biol.)
Schaefer H F (Theoretical Chem.)
Shirley D A (Physical Chem.)
Stephens F S (Theoretical Chem.)
Trilling G H (Physics)

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U. of Cal. (cont'd.)

San Francisco

Arnaud C D (Endocrinology)
Bishop J M (Virology)
Cleaver J E (Genetics)
Forsham P H (Endocrinology)
Goodman H M (Molec. Biol.)
Grodsky G M (Endocrinology)
Grumbach M M (Endocrinology)
Kaplan S L (Endocrinology)
Li C H (Biochemistry)
McCarthy B J (Microbiol.)
Papahadjopoulos D P (Molec. Biol.)
Parmley W W (Cardiology)
Rector F C (Physiology)
Rudolph A M (Physiology)
Rutter W J (Molec. Biol.)
Stoeckenius W (Biophysics)
Tomkins G M (Biochemistry)

San Diego

Butler W L (Biophysics)
Covell J W (Physiology)
Dutton R W (Immunology)
Helinski D R (Genetics)
Hofmann A F (Gastroenterol.)
Kaplan N O (Enzymology)
Kearns D R (Biophysics)
O'Brien J S (Genetics)
Ross J (Cardiology)
Schrauzer G N (Inorganic Chem.)
Seegmiller J E (Immunology)
Sell S (Immunology)
Singer S J (Cell Biol.)
Steinberg D (Biochemistry)
Yen S S C (Endocrinology)

Los Angeles

Atkinson D E (Biochemistry)
Chopra I J (Endocrinology)
Cline M J (Hematology)

Stanford (cont'd.)

Bulos F (Physics)
Drell S D (Physics)
Feldman G J (Physics)
Fryberger D (Physics)
Hanson G (Physics)
Larsen R R (Physics)
Luth V (Physics)
Lynch H L (Physics)
Paterson J M (Physics)
Perl M L (Physics)
Richter B (Physics)

School of Medicine

Bensch K G (Pathology)
Berg P (Molec. Biol.)
Cohen S N (Molec. Biol.)
Goldstein A (Pharmacology)
Harrison D C (Cardiology)
Herzenberg L A (Immunology)
Kaplan H S (Oncology)
Kornberg A (Molec. Biol.)
McDevitt H O (Immunology)
Melmon K L (Cell Biol.)
Merigan T C (Virology)
Remington J S (Microbiol.)
Rosenberg S A (Oncology)

Department of Chemistry

Collman J P (Inorganic Chem.)
Djerassi C (Organic Chem.)
Flory P J (Physical Chem.)
McConnell H M (Biophysics)
Taube H (Inorganic Chem.)

Department of Biological Sciences

Schimke R T (Molec. Biol.)
Wessels N K (Cell Biol.)
Yanofsky C (Molec. Biol.)

Department of Electrical Engineering

Spicer W E (Physics)

University of Southern California, Los Angeles (7)

Benson S W (Physical Chem.)
Engel W K (Neurology)
Heidelberger C (Oncology)
Massry S G (Physiology)
Olah G A (Organic Chem.)
Segal G A (Theoretical Chem.)
Vogt P K (Virology)

City of Hope Research Institute, Duarte (2)

Comings D E (Cell Biol.)
Ohno S (Genetics)

La Jolla Cancer Research Foundation (2)

Fishman W H (Oncology)
Ruoslahti E I (Oncology)

Cedars Sinai Medical Center, Los Angeles (1)

Swan H J C (Physiology)

Helicon Foundation, San Diego (1)

Thomas C A (Genetics)

Hewlett Packard, Palo Alto (1)

Morehouse C C (Physics)

Pomona College, Claremont (1)

Hansch C (Organic Chem.)

Syntex Research, Palo Alto (1)

Allison A C (Immunology)

Colorado (12)

University of Colorado (5)

Claman H N (Immunology)
Porter K R (Molec. Biol.)
Schalch D S (Endocrinology)
Starzl T E (Immunology)
Tan E M (Immunology)

University of Connecticut, Farmington (2)

Raisz L G (Endocrinology)
Ward P A (Pathology)

Florida (6)

University of Florida, Gainesville (4)

Hayflick L (Cell Biol.)
Katritzky A R (Organic Chem.)
McGuigan J E (Gastroenterol.)
Winefordner J D (Analyt. Chem.)

University of Miami (2)

Parks W P (Immunology)
Pressman B C (Pharmacology)

Georgia (6)

Emory University, Atlanta (2)

Kuo J F (Pharmacology)
Nahmias A J (Virology)

University of Georgia, Athens (2)

Allinger N L (Organic Chem.)
King R B (Organomet. Chem.)

Georgia Institute of Technology, Atlanta (1)

House H O (Organic Chem.)

Medical College of Georgia, Augusta (1)

Huisman T H J (Biochemistry)

Illinois (25)

University of Illinois (11)

Urbana-Champaign

Drago R S (Inorganic Chem.)
Flygare W H (Chem. Phys.)
Hageman R H (Plant Sciences)
Iben I (Astrophysics)
Leonard N J (Biochemistry)
Sah C T (Physics)

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Mickey M R (Immunology)
Morton D L (Oncology)
Sawyer C H (Endocrinology)
Smith E L (Biochemistry)
Terasaki P I (Immunology)
Winstein S (Physical Chem.)

Davis
Bradbury E M (Cell Biol.)
Mason D T (Cardiology)
Spurr A R (Plant Sciences)
Tappel A L (Biochemistry)
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Los Alamos, New Mexico
Bame S J (Astrophysics)
Cromer D T (Physics)
Mann J B (Physics)

Santa Barbara
Baker B R (Organic Chem.)
Bruice T C (Organic Chem.)
Fearson R G (Inorganic Chem.)

Irvine
Hehre W J (Theoretical Chem.)

Riverside
Norman A W (Biochemistry)

Torrance
Fisher D A (Endocrinology)

Stanford University, Palo Alto (35)
Stanford Linear Accelerator Center
Boyarski A M (Physics)
Breidenbach M (Physics)

California Institute of Technology, Pasadena (12)
Division of Chemistry and Chemical Engineering
Davidson N R (Molec. Biol.)
Gray H B (Inorganic Chem.)
Marcus R A (Physical Chem.)
Roberts J D (Organic Chem.)
Vinograd J (Molec. Biol.)

Division of Biology
Attardi G (Molec. Biol.)
Bonner J F (Genetics)
Davidson E H (Molec. Biol.)
Sinsheimer R L (Molec. Biol.)

Hale Observatory
Oke J B (Astronomy)
Sandage A (Astronomy)

Kerckhoff Marine Laboratory, Corona-Del-Mar
Britten R J (Molec. Biol.)

Salk Institute, La Jolla (7)
Bloom F E (Neuropharmacol.)
Cohn M (Immunology)
Cowan W M (Neurology)
Dulbecco R (Oncology)
Guillemin R (Endocrinology)
Rivier J (Endocrinology)
Vale W W (Endocrinology)

Scripps Clinic, La Jolla (7)
Beutler E (Hematology)
Cochrane C G (Immunology)
Dixon F J (Immunology)
Katz D H (Immunology)
Muller-Eberhard H J (Immunology)
Reisfeld R A (Immunology)
Weigle W O (Immunology)

National Oceanic & Atmospheric Administration, Boulder (3)
Fehsenfeld F C (Geophysics)
Ferguson E E (Aeronomy)
Schmeltekopf A L (Physics)

Colorado State University, Ft. Collins (2)
Niswender G D (Endocrinology)
Venable J H (Cell Biol.)

Denver General Hospital (1)
Moore G E (Oncology)

National Jewish Hospital, Denver (1)
Grey H M (Pathology)

Connecticut (17)
Yale University, New Haven (15)
School of Medicine
Aghajanian G K (Neuropharmacol.)
Curran P F (Biophysics)
Farquhar M G (Cell Biol.)
Felig P (Endocrinology)
Gershon R K (Immunology)
Greengard P (Cell Biol.)
Marchesi V T (Pathology)
Palade G E (Cell Biol.)
Rasmussen H (Cell Biol.)
Roth R H (Pharmacology)
Shulman R G (Chem. Phys.)
Waksman B H (Immunology)

Department of Biology
Gall J G (Molec. Biol.)
Ruddle F H (Genetics)

Department of Chemistry
Wiberg K B (Organic Chem.)

U. of Ill. (cont'd.)
Chicago
Dray S (Immunology)
Lands W E M (Biochemistry)
Rosen K M (Cardiology)
Spellacy W N (Endocrinology)
Walier R (Physiology)

University of Chicago (7)
Halpern J (Inorganic Chem.)
Rabinowitz M (Molec. Biol.)
Rice S A (Physical Chem.)
Roizman B (Virology)
Steck T L (Biochemistry)
Steiner D F (Endocrinology)
Taylor E W (Biophysics)

Northwestern University, Evanston (3)
Bender M (Organic Chem.)
Ibers J A (Inorganic Chem.)
Waber J T (Physics)

Fermi National Accelerator Laboratory, Batavia (2)
Bjorken J D (Physics)
Lee B W (Physics)

Illinois Mental Health Institutes, Chicago (1)
Davis J M (Psychiatry)

Rush-Presbyterian-St. Luke's Medical Center, Chicago (1)
Gewurz H (Immunology)

Indiana (10)
Indiana University (5)
Allerhand A (Biophysics)
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Indiana U. (cont'd.)

Kochi J K (Organomet. Chem.)
Mahler H R (Cell Biol.)

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Altman J (Neurology)
Brown H C (Organic Chem.)
Cooks R G (Analyt. Chem.)
Morre D J (Plant Sciences)

University of Notre Dame (1)
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Chalkley R (Molec. Biol.)

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Arimura A (Endocrinology)
Bowers C Y (Endocrinology)
Burch G E (Cardiology)
Coy D H (Endocrinology)

Maryland (96)
National Institutes of Health (NIH), Bethesda (79)
National Cancer Institute (NCI)
Aaronson S A (Oncology)
Borsos T (Immunology)
De Vita V T (Oncology)
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Chanock R M (Virology)
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Rowe W P (Virology)
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Costa E (Neuropharmacol.)
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Massachusetts (85)

Harvard University, Cambridge (43)
Medical School, Boston
Austen K F (Immunology)
Benacerraf B (Immunology)
Braunwald E (Cardiology)
Cantor H (Immunology)
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Hubel D H (Physiology)
Ingbar S H (Endocrinology)
Karnovsky M J (Cell Biol.)
Merrill J P (Immunology)
Richardson C C (Molec. Biol.)
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Schildkraut J J (Psychiatry)
Schur P H (Immunology)
Smith T W (Cardiology)
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Weinstein L (Microbiol.)
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MIT (cont'd.)

Lodish H F (Cell Biol.)
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Department of Chemistry
Seyferth D (Organomet. Chem.)
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Department of Nutrition and Food Science
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Goldstein I J (Biochemistry)

444

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Albany
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Brooklyn
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Schlessinger D (Molec. Biol.)
Shreffler D C (Genetics)
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Green M (Molec. Biol.)

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New Jersey (30)

Roche Institute for Molecular Biology, Nutley (7)
Horecker B L (Biochemistry)
Kaback H R (Molec. Biol.)
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Shatkin A J (Virology)
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Remeika J P (Physics)
Van Uitert L G (Physics)

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Gross D J (Physics)
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New York (85)
Rockefeller University (14)
Allfrey V G (Biochemistry)
Blobel G K J (Cell Biol.)
Choppin P W (Virology)
Cohn Z A (Cell Biol.)
Darnell J E (Cell Biol.)
de Duve C R (Cell Biol.)
Edelman G M (Immunology)
Kappas A (Pharmacology)
Kunkel H G (Immunology)
Lipmann F (Biochemistry)
Mirsky A E (Cell. Biol.)
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Columbia University (11)
College of Physicians & Surgeons
Benesch R (Biochemistry)
Benesch R E (Biochemistry)
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Kabat E A (Immunology)
Spiegelman S (Molec. Biol.)

Department of Chemistry
Bernstein R B (Physical Chem.)
Cantor C R (Biochemistry)
Nakanishi K (Organic Chem.)
Turro N J (Organic Chem.)

Department of Physics
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Cornell University, Ithaca (11)

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Sonnenblick E H (Cardiology)
Suzuki K (Neurology)

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Goldstein M (Neurology)
Krugman S (Virology)
Nussenzweig V (Immunology)
Shapiro A L (Biophysics)
Weissmann G (Cell Biol.)

Mount Sinai Medical Center (5)
Gorlin R (Cardiology)
Hirschhorn K (Genetics)
Holland J F (Oncology)
Popper H (Pathology)
Schaffner F (Pathology)

Brookhaven National Laboratory, Upton (4)
Hamilton W C (Physical Chem.)
Setlow R B (Biophysics)
Shirane G (Physics)
Studier F W (Molec. Biol.)

Memorial Sloan-Kettering Cancer Center (3)
Boyse E A (Immunology)
Friedberg C E (Physics)
Old L J (Oncology)

Roswell Park Memorial Institute, Buffalo (3)
Henderson E S (Oncology)
Pressman D (Immunology)
Sandberg A A (Cell Biol.)

North Carolina (11)
Duke University Medical Center, Durham (7)
Amos D B (Immunology)
Fridovich I (Enzymology)
Hill R L (Biochemistry)
Joklik W K (Virology)
Lefkowitz R J (Pharmacology)
Reynolds J A (Biophysics)
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Breese G R (Pharmacology)
Eliel E L (Organic Chem.)
Utiger R D (Endocrinology)

Burroughs Wellcome Co., Research Triangle Park (1)
Cuatrecasas P (Biochemistry)

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Case Western Reserve University, Cleveland (2)
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446

Oklahoma (2)

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Good R A (Immunology)

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Exton J H (Endocrinology)
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Veteran's Administration (13)
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Yalow R S (Endocrinology)

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Fefer A (Immunology)
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Ross R (Cell Biol.)
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Hellstrom I (Immunology)
Hellstrom K E (Immunology)
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Jde 7

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Knobil E (Endocrinology)
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Robison G A (Pharmacology)
Ross G T (Endocrinology)
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tions they produced. They found that this measure of "productivity" correlated significantly with several variables they analyzed—total university revenue, faculty size, number of books in the library, number of undergraduate and graduate students, and even their scholastic aptitude scores. However, only 14 of the top 20 most productive universities they identified also appear among the top 20 US, UK, and Canadian institutions on our list, ranked by number of most-cited authors.

They also found that these variables could be predicted simply by knowing how many psychology faculty members at each institution were cited more than 25 times in 1977.[6] Unfortunately, they didn't rank the universities by this measure of "impact." Instead, they provided aggregate data. It would have been interesting to compare their list with ours, since both are based on citation data. In a future study, we'll make a proper comparison between institutional rankings by total publications and by the number of citations they receive.

In closing, I'd like to point out an error of omission we made in Part 2D of this study.[5] We neglected to include Rosalyn S. Yalow among the endocrinologists on the list who won the Nobel prize. She shared the 1977 Nobel with R. Guillemin and A. V. Schally, both of whom also appear on the list in Part 2D.

* * * * *

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6. **Rushton J P & Meltzer S.** Research productivity, university revenue, and scholarly impact (citations) of 169 British, Canadian, and United States universities (1977). *Scientometrics* 3:275-303, 1981.

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Current Comments®

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The 1984 Nobel Prize in Medicine Is Awarded to Niels K. Jerne, César Milstein, and Georges J.F. Kohler for Their Contributions to Immunology

Number 45 November 11, 1985

The Nobel Assembly of Karolinska Institute awarded the 1984 Nobel Prize in physiology or medicine jointly to Niels K. Jerne, professor emeritus, Basel Institute for Immunology, Switzerland, César Milstein, British Medical Research Council's Laboratory of Molecular Biology, Cambridge, UK, and Georges J.F. Köhler, now at the Max Planck Institute for Immunobiology, Freiburg, Federal Republic of Germany. These scientists were honored for their extraordinary contributions to the field of immunology—Jerne for his theoretical advances that have shaped our concepts of the immune system and Milstein and Köhler for the production of monoclonal antibodies using the hybridoma technique, a methodological breakthrough with broad practical significance.

My discussion on this year's Nobel Prize winners will be in three parts. Next week's essay will focus on the Nobel Prize winners in physics and chemistry. Physicists Carlo Rubbia and Simon van der Meer of CERN, Geneva, Switzerland, shared the physics award for their contributions to the discovery of the heavy field particles W and Z. Biochemist R. Bruce Merrifield, Rockefeller University, New York, was awarded the chemistry prize for his simplification of the method for producing complex proteins. The third essay will discuss the Nobel Prize in literature, awarded to Czechoslovakian poet Jaroslav Seifert, and the Alfred Nobel Memorial Prize in Economic Science, given to Sir Richard Stone, University of Cambridge, UK, for his pioneering national accounts system.

Through citation analysis these essays emphasize the role that scientific literature plays in the discovery process. My previous essays on Nobel Prize winners illustrate how citation analysis can provide a documentary perspective on the field the Nobel award has recognized.[1,2] To establish this perspective, we locate the relevant research fronts on the worldwide map of science. A research front is simply a group of current papers that cite one or more papers in a cluster of core papers.[3]

I am often asked if we can anticipate the winners of the Nobel Prizes. By examining strings of annual research fronts over a long period of time, one can reasonably *forecast* fields that may one day be recognized by a Nobel Prize. Likewise, a long-term analysis of the publication and citation records of notable scientists within these forecasted fields shows who are highly likely to be among future Nobel Prize winners if that field is recognized.

Using this methodology, it is not surprising that our published lists of most-cited contemporary scientists have included many Nobel Prize winners. In Table 1 we have listed those winners who were among the 1,000 most-cited authors for 1965 to 1978. Note that many of these winners were listed before the prizes were awarded. However, without being privy to the rationale involved in the specific annual decisions of the Nobel Assembly, it is impossible to *predict* either the fields or the persons to be recognized.

The 1984 Nobel Prize pays tribute to the field of immunology, the study of the

Table 1: Nobel Prize winners who were among the 1,000 most-cited contemporary scientists, 1965-1978. A = name. B = field. C = year Nobel Prize was won.

A	B	C
Anderson PW	physics	1977
Anfinsen CB	biochemistry	1972
Axelrod J	pharmacology	1970
Baltimore D	virology	1975
Barton DHR	organic chemistry	1969
Benacerraf B	immunology	1980
Berg P	molecular biology	1980
Bloembergen N	physics	1981
Blumberg BS	oncology	1976
Brown HC	organic chemistry	1979
Dausset J	immunology	1980
DeDuve C	cell biology	1974
Edelman GM	immunology	1972
Fischer EO	organometallic chemistry	1973
Flory PJ	physical chemistry	1974
Glashow SL	physics	1979
Guillemin R	endocrinology	1977
Hoffmann R	inorganic chemistry	1981
Hubel DH	physiology	1981
Jacob F	molecular biology	1965
Katz B	biophysics	1970
Khorana HG	organic chemistry	1968
Klug A	molecular biology	1982
Kornberg A	molecular biology	1959
Krebs HA	biochemistry	1953
Lee TD	physics	1952
Lipmann F	biochemistry	1953
Lipscomb WN	physical chemistry	1976
Milstein C	immunology	1984
Mitchell PD	biochemistry	1978
Monod JL	molecular biology	1965
Mott NF	physics	1977
Nathans D	microbiology	1978
Nirenberg M	genetics	1968
Ochoa S	biochemistry	1959
Palade GE	cell biology	1974
Perutz MF	molecular biology	1962
Richter B	physics	1976
Rubbia C	physics	1984
Samuelsson B	biochemistry	1982
Sanger F	molecular biology	1958 & 1980
Schally AV	endocrinology	1977
Sutherland EW	pharmacology	1971
Taube H	inorganic chemistry	1983
Temin HM	oncology	1975
Vane JR	pharmacology	1982
Weinberg S	physics	1979
Wiesel TN	physiology	1981
Wilkinson G	organometallic chemistry	1973
Wilson KG	physics	1982
Woodward RB	organic chemistry	1965

body's ability to resist invasion by foreign organisms or substances. Immunology is a far-ranging field including research from a variety of smaller biological topics such as genetics and virology. As a result, immunology has had a tremendous impact on the research community. Its value is rarely questioned, particularly with the outbreak of new diseases, such as the deadly—and as yet incurable—Acquired Immunodeficiency Syndrome (AIDS). While space cannot permit a complete discussion of immunology, the breadth of the topic compels me to spend more time discussing this topic than usual.

Antibodies and Antigens

Part of the body's power to fight off intruders resides in antibodies. Produced by white blood cells called lymphocytes, each antibody is made to recognize a specific antigenic determinant, a specific site found on a foreign substance. The antibody binds with the antigen in a lock-and-key fashion to inhibit the antigen's biologic activity and mark it for destruction by scavenger cells. After the invader has been neutralized, many antibodies remain in the blood so that the next time the same antigen-bearing molecule invades, the body's defenses are already mobilized.

In his Nobel Prize acceptance speech given in Stockholm, Jerne compared the immune system to language to explain the large variety of foreign invaders that the immune response can handle. "Looking at languages, we find that all of them make do with a vocabulary of roughly a hundred thousand words or less. These vocabulary sizes are a hundred times smaller than the estimates of the size of the antibody repertoire available to our immune system."[4]

Table 2 provides a brief survey of some of the outstanding contributions to the early development of immunology prior to the twentieth century.

From 1900 through 1950, immunological research focused on the underlying mechanisms in the immune response. During this time it was generally supposed that the antigen served as a template on which antibodies were formed. Yet this theory did not explain the presence of antibodies in the serum

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Table 2: A selective history of immunology studies

Scientist	Year	Contribution to Immunology
Edward Jenner	1798	Developed first effective immunization procedure using virulent cowpox against smallpox infection
Louis Pasteur	1881	Demonstrated that vaccination procedure can be applied to many microbial infections. Caused vaccination to gain widespread acceptance
Paul Ehrlich	1897	Proposed side-chain theory of antibody synthesis and promoted his idea that antibody specificity results from antigen recognizing an antibody with complementary molecular structure and that antigen-antibody interaction is a chemical union
Jules Bordet	1899	Found that antibodies can destroy non-threatening cell types in addition to pathogenic bacteria
Karl Landsteiner	1900	Showed that an antibody can distinguish between subtly different antigenic structures

Table 3: Jerne's most-cited papers in 1955-1984 *SCI*®. A = number of citations. B = bibliographic data. *SCI*/*SSCI*® research front(s) to which the paper is core follows B.

A	B
2400	**Jerne N K & Nordin A A.** Plaque formation in agar by single antibody-producing cells. *Science* 140:405, 1963. 83-7618
1671	**Jerne N K, Nordin A & Henry C.** The agar plaque technique for recognizing antibody-producing cells. (Amos B & Koprowski H, eds.) *Cell-bound antibodies.* Philadelphia: Wistar Institute Press, 1963. p. 109-25.
966	**Jerne N K.** Towards a network theory of the immune system. *Ann. Immunol.—Inst. Pasteur* C125:373-89, 1974. 76-1010, 83-5673, 84-0032
676	**Jerne N K.** The somatic generation of immune recognition. *Eur. J. Immunol.* 1:1-9, 1971. 81-0016, 82-0317, 83-1491
300	**Jerne N K.** The natural-selection theory of antibody formation. *Proc. Nat. Acad. Sci. US* 41:849-57, 1955.
277	**Jerne N K, Henry C, Nordin A A, Fuji H, Koros A M C & Lefkovits I.** Plaque forming cells: methodology and theory. *Transplant. Rev.* 18:130-91, 1974.
272	**Rajewsky K, Schirrmacher V, Nase S & Jerne N K.** The requirement of more than one antigenic determinant for immunogenicity. *J. Exp. Med.* 129:1131-43, 1969. 75-1212
223	**Henry C & Jerne N K.** Competition of 19S and 7S antigen receptors in the regulation of the primary immune response. *J. Exp. Med.* 128:133-52, 1968.
212	**von Boehmer H, Haas W & Jerne N K.** Major histocompatibility complex-linked immune-responsiveness is acquired by lymphocytes of low-responder mice differentiating in thymus of high-responder mice. *Proc. Nat. Acad. Sci. US* 75:2439-42, 1978. 81-0016, 82-0317, 83-1491
139	**Jerne N K.** The immune system: a web of V-domains. *Harvey Lect.* 70:93-110, 1976.

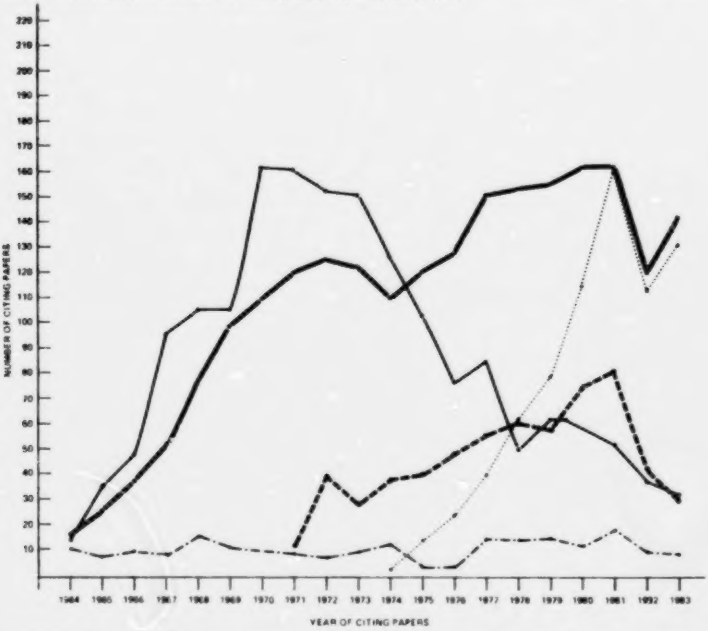
of normal animals. This problem became the focus of studies by Niels K. Jerne.

Niels K. Jerne

Born in London in 1911, Jerne grew up in Holland where he began his studies in physics. After two years, he switched to medicine at the University of Copenhagen, Denmark, where he began studying antibodies. From 1943 to 1956, Jerne continued his studies on antibodies at the Danish State Serum Institute. From there, he spent six years in Geneva, working at the World Health Organization and later at the University of Geneva.

He was chairman of the Department of Microbiology at the Medical School of the University of Pittsburgh between 1962 and 1966, and it was during this period that Jerne and Albert A. Nordin, also of the University of Pittsburgh, described a simple method for counting specific antibody-forming cells among a mixed-cell population. Their paper "Plaque formation in agar by single antibody-producing cells" has been cited over 2,400 times since its publication in 1963, making it Jerne's most highly cited paper. This method has since become a routine technique.[5]

Figure 1: Chronological distribution of citations to Niels Jerne's five most-cited papers. The heavy solid line shows citations to his 1963 *Science* paper; light solid line shows citations to the 1963 paper in *Cell-Bound Antibodies*; dots show citations to the 1974 *Annales d'Immunologie* paper; dashes show citations to 1971 *European Journal of Immunology* paper; dot-dash line shows citations to 1955 *Proceedings of the National Academy of Sciences of the United States of America* paper.



In 1966 Jerne left the US to become director of the Paul Ehrlich Institute in Frankfurt. In 1969 when the Basel Institute of Immunology was founded, Jerne became its first director. During his tenure, he became widely acknowledged as a leader in immunological theory. The Nobel Assembly has recognized Jerne's most influential theories as deserving of the Nobel Prize, noting, "In three main theories he has in a visionary way elucidated essential questions concerning specificity, development and regulation of the immune response."[6]

His first notable theory was proposed in 1955. In his paper "The natural-selection theory of antibody formation," Jerne abandoned the old antigen-template theories and proposed that antibody molecules are already present in the host, having developed during fetal life. An invading foreign antigen selects the antibody molecule that has the best fit for binding. This antigen-antibody binding stimulates production of the chosen antibody.[7]

In addition to providing sharp contrast to the prevailing theories, Jerne's natural-selection theory was the first to forge the idea that a specialized population of cells, now known as lymphocytes, is required for antibody formation. His hypothesis was later proven to be true and today is considered to be the theoretical foundation for immunology.

In his theory on "The somatic generation of immune recognition," published in 1971, Jerne described how antibodies

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are developed in the fetus before foreign antigens are encountered.[8] Observing that a single animal can make a very large number of different antibodies, Jerne proposed that antigen-sensitive cells (lymphocytes committed to making antibodies) must have a diversity of antibody genes. The antigen-sensitive cells, stimulated by a self-antigen (an antigen present within the animal), enter cell division. Mutations occur in the rapidly dividing cells, producing a pool of antibodies with new immunological specificities capable of handling any foreign antigen. Meanwhile, the specificities of the animal's antigen-sensitive cells for its own antigens are weakened. This theory explains why individual animals of the same species produce different antibodies when stimulated by the same antigen and why animals usually do not make antibodies against their own antigens. According to Jerne's theory, all animals have different self-antigens, and further diversity results from the random mutation process that generates the final antibody pool.

Jerne's third influential theory, proposed in 1974, attempts to explain how the specific immune response is regulated. "Towards a network theory of the immune system"[9] postulates that idiotypes (antigenic markers found on antibodies) may act as antigens eliciting an antibody response. These new antibodies, called anti-antibodies, also contain idiotypes, which in turn may elicit another antibody response. This cascading effect forms a network between the cells of the immune system. Under normal conditions, Jerne believes the network is balanced, but the equilibrium becomes disturbed when an antigen is introduced. To restore balance, Jerne proposes that the immune system causes a response to the antigen that results in the cascading effect and the production of anti-antibodies.

Hybridoma Technique Development

Jerne's theoretical work laid the foundation for many advances in immunology, including the monoclonal antibody production technique developed by César Milstein and Georges Köhler in 1975. In their announcement of the Nobel Prize, the Karolinska Institute described this discovery as "one of the most important methodological advances in biomedicine during the 1970's."[6]

César Milstein was born in Argentina in 1927. He began working at the National Institute of Microbiology in Buenos Aires in 1957. Between 1958 and 1961, he took a leave of absence from the institute to earn his doctorate at the Department of Biochemistry, University of Cambridge, UK, after which he returned to Buenos Aires. In 1963 he left Argentina to join the staff of the Medical Research Council Laboratory of Molecular Biology in Cambridge, where today he heads the Division of Protein and Nucleic Acid Chemistry. Over the past 20 years, Milstein has pioneered in the study of genetic variation among antibodies[10,11] as well as the intracellular mechanisms underlying antibody synthesis.[12]

Georges J.F. Köhler, born in 1946 in Munich, received his doctoral degree in biology from the University of Freiburg in 1974. His doctoral work focused on his findings that the immune system of the mouse can produce a thousand different kinds of antibody molecules against a single antigenic site on an antigen. To study the mutations in the genes that specify antibodies, he began postgraduate research in Milstein's laboratory in Cambridge. It was during this period that Köhler and Milstein developed the technique for monoclonal antibody formation.

Prior to their studies, antibodies specific for a particular antigen could be made only by injecting an animal with a chosen antigen, removing blood from the animal, and separating the antiserum, that portion of blood containing antibodies. This technique was laborious, since the immune system responds to minute amounts of antigen. Even if the purest antigen is used, extra antibodies are produced as a result of trace amounts of contaminants. In order to obtain the antibodies specific for the injected antigen, unwanted antibodies have to be identified and eliminated, a difficult and time-consuming task.

Another problem resulted from the fact that lymphocytes, the producers of antibodies, are highly specialized cells that lack the ruggedness needed to survive in a tissue-culture medium. Therefore, even if the lymphocyte that made a specific antibody is identified, it is unable to survive the rigors of the laboratory processes that would be required to isolate it and use it as a source of pure antibody.

Milstein and Köhler recognized the need for the long-term growth of specific antibody-forming cells in culture. The work by Michael Potter in 1962 at the National Cancer Institute, Bethesda, Maryland, had already established that myelomas (tumor cells) can be cultivated indefinitely.[13] The fusing of two different cell types had been performed earlier by R.G.H. Cotton, Royal Children's Hospital Research Foundation, Victoria, Australia, and Milstein, who fused rat and mouse myeloma cells.[14] Building upon these earlier studies, Köhler and Milstein produced a hybridoma by fusing myeloma cells with lymphocytes immunized by a selected antigen.[15]

In a hybridoma, the myeloma cell contributes the capacity for survival, while the lymphocyte allows production of antibodies with specificity for certain types of antigens. These hybridomas can be propagated, and the clones that result can provide unlimited production of a highly specific antibody. These specific antibodies are called monoclonal antibodies and have become an essential tool in biomedical research. The paper describing this technique has been cited over 3,000 times since its publication in 1975, making it the most cited of many highly cited papers by both Köhler and Milstein.

It is interesting to note that Köhler and Milstein originally developed this technique to study the rate of mutation of antibody genes to further understand antibody diversity. Milstein, quoted in a *Science News* article, said, "We were working on very esoteric questions. Only later we realized the [general] potential of the procedures."[16] In their paper describing the hybridoma technique, Köhler and Milstein appeared to underestimate the ramifications of their discovery when they modestly concluded that "it is possible to hybridise antibody-producing cells from different origins. Such cells can be grown *in vitro* in massive cultures to provide specific antibody. Such cultures could be valuable for medical and industrial use."[15]

Monoclonal Antibody Applications

Today, monoclonals are having a strong impact on most areas of medicine. A major application for monoclonals is in cancer therapy. One of the limitations of standard anticancer therapies is that cancer drugs and radiation therapy attack both cancerous and healthy cells. However, Robert W. Baldwin and Malcolm V. Pimm, Cancer Research Campaign Laboratories, University of Nottingham, UK, are experimenting with an approach in which monoclonals can be prepared to bind with a specific cancer antigen. By injecting a patient with a cancer drug carried by a monoclonal specific for the cancer type, the monoclonal can lead the drug directly to the cancer cells, sparing the healthy cells from the effects of the drug.[17]

Monoclonals can also make bone-marrow transplants more practical for people suffering from leukemia. Jerome Ritz, Division of Tumor Immunology, Sidney Farber Cancer Institute, Boston, Massachusetts, describes an approach that uses monoclonals to selectively deplete the malignant cells from the bone marrow prior to autologous or self-transplantation of the marrow. In addition, monoclonals can be used to promote marrow transplantation between a donor and recipient who are not normally histocompatible—a state of mutual tol-

Table 4: Milstein's most-cited papers in 1955-1984, *SCI*®. A = number of citations received. B = bibliographic data. *SCI/SSCI*® research front(s) for which the paper is core follows B.

A	B
3040	**Köhler G & Milstein C.** Continuous cultures of fused cells secreting antibody of predefined specificity. *Nature* 256:495-7, 1975. 78-1440, 79-0363, 81-1664
811	**Köhler G & Milstein C.** Derivation of specific antibody-producing tissue culture and tumor lines by cell fusion. *Eur. J. Immunol.* 6:511-9, 1976. 78-1440, 79-0363, 81-1664
693	**Galfre G, Howe S C, Milstein C, Butcher G W & Howard J C.** Antibodies to major histocompatibility antigens produced by hybrid cell lines. *Nature* 266:550-2, 1977. 78-1440, 79-0363, 81-1664
490	**Milstein C, Brownlee G G, Harrison T M & Mathews M B.** A possible precursor of immunoglobulin light chains. *Nature—New Biol.* 239:117-20, 1972. 74-0003, 75-0034, 76-0514, 77-0621, 78-0534, 79-0550, 81-0054, 82-0224
346	**Köhler G, Howe S C & Milstein C.** Fusion between immunoglobulin-secreting and nonsecreting myeloma cell lines. *Eur. J. Immunol.* 6:292-5, 1976.
324	**Williams A F, Galfre G & Milstein C.** Analysis of cell surfaces by xenogeneic myeloma-hybrid antibodies: differentiation antigens of rat lymphocytes. *Cell* 12:663-73, 1977. 79-0363, 82-1232, 83-0081, 84-3435
313	**Barnstable C J, Bodmer W F, Brown G, Galfre G, Milstein C, Williams A F & Ziegler A.** Production of monoclonal antibodies to group A erythrocytes, HLA and other human cell surface antigens—new tools for genetic analysis. *Cell* 14:9-20, 1978. 79-0363, 81-0710, 82-0788, 83-1880
257	**McMichael A J, Pilch J R, Galfre G, Mason D Y, Fabre J W & Milstein C.** A human thymocyte antigen defined by a hybrid myeloma monoclonal antibody. *Eur. J. Immunol.* 9:205-10, 1979. 81-0136, 84-0171
220	**Milstein C.** Linked groups of residues in immunoglobulin k chains. *Nature* 216:330-2, 1967.
178	**Springer T, Galfre G, Secher D S & Milstein C.** Mac-1: a macrophage differentiation antigen identified by monoclonal antibody. *Eur. J. Immunol.* 9:301-6, 1976. 82-0673

Table 5: Köhler's most-cited papers in 1955-1984, *SCI*®. A = citations. B = bibliographic data. *SCI/SSCI*® research front(s) for which the paper is core follows B.

A	B
3040	**Köhler G & Milstein C.** Continuous cultures of fused cells secreting antibody of predefined specificity. *Nature* 256:495-7, 1975. 78-1440, 79-0363, 81-1664
811	**Köhler G & Milstein C.** Derivation of specific antibody-producing tissue culture and tumor lines by cell fusion. *Eur. J. Immunol.* 6:511-9, 1976. 78-1440, 79-0363, 81-1664
427	**Shulman M J, Wilde C D & Köhler G.** A better cell line for making hybridomas secreting specific antibodies. *Nature* 276:269-70, 1978.
346	**Köhler G, Howe S C & Milstein C.** Fusion between immunoglobulin-secreting and nonsecreting myeloma lines. *Eur. J. Immunol.* 6:292-5, 1976.
63	**Köhler G, Hengartner H & Shulman M J.** Immunoglobulin production by lymphocyte hybridomas. *Eur. J. Immunol.* 8:82-8, 1978.
47	**Köhler G, Lefkovits I, Elliott B & Coutinho A.** Derivation of hybrids between a thymoma line and spleen cells activated in a mixed leukocyte reaction. *Eur. J. Immunol.* 7:758-61, 1977.
44	**Milstein C, Adetugbo K, Cowan N J, Köhler G, Secher D S & Wilde C D.** Somatic cell genetics of antibody-secreting cells: studies of clonal diversification and analysis by cell fusion. *Cold Spring Harbor Symp.* 41:793-803, 1976.
43	**Forni L, Coutinho A, Köhler G & Jerne N K.** IgM antibodies induce the production of antibodies of the same specificity. *Proc. Nat. Acad. Sci. US* 77:1125-8, 1980.
41	**Köhler G, Pearson T & Milstein C.** Fusion of T and B cells. *Somatic Cell Genet.* 3:303-12, 1977.
35	**Köhler G.** Frequency of precursor cells against the enzyme β-galactosidase. An estimate of the BALB/c strain antibody repertoire. *Eur. J. Immunol.* 6:340-7, 1976.

erance allowing different tissues to be grafted together.[18]

In kidney transplant recipients, the early signs of infection are identical to those of impending organ rejection. Y.C. Smart, Faculty of Medicine, University of Newcastle, Australia, and colleagues have found that monoclonals may accurately diagnose infection versus rejection, which is imperative because these two problems must be treated in different ways.[19]

Major breakthroughs often become the source for new perplexing research questions. While researchers realize the potential benefits that can be derived from monoclonals, Henry Krakauer, National Institute of Allergy and Infectious Diseases, NIH, Bethesda, and Office of Research, Health Care Financing Administration, Baltimore, Maryland, notes that research using monoclonal antibodies has unveiled deterring problems.[20] For example, research is being done to develop methods for obtaining hybridomas that produce human antibodies. However, antibody-producing human lymphocytes fused to mouse myeloma cells have proved unstable; suitable human myeloma cells need to be developed for fusion. In addition, it is not ethical or legal for humans to be injected with viruses or cancer cells to provoke an immune response, making it difficult to obtain human lymphocytes that produce a desired antibody.

Nevertheless, monoclonal research has hinted at significant potential. In 1983 NIH funded 358 research projects in which monoclonal antibodies were either the principal objects of the investigations or the essential tools.[20]

Citation Studies

Although Jerne was honored by the Nobel Committee for his theoretical work, it is interesting, but hardly surprising, to note that his methodological paper "Plaque formation in agar by single antibody-producing cells"[5] is his most-cited paper (Table 3). Of interest to citation analysts is the fact that this article is one of the three most-cited papers published in *Science* from 1961 to 1980.[21] As we reported earlier, it is in fact one of the most-cited papers of all time.[22] Jerne discussed this paper in a *Citation Classic* commentary in 1981, long before being awarded the Nobel Prize.[23] The chronological frequency graph for this paper and his four other most-cited papers is shown in Figure 1. Within five years of publication, this paper had already become a citation classic. In 1966 one might have forecast the growth of its impact.

Jerne's three theories recognized by the Nobel Assembly are discussed in his third, fourth, and fifth most highly cited papers. His natural-selection theory[7] has been cited 300 times, his paper on somatic generation[8] has been cited over 675 times, and his paper on the network theory[9] has been cited over 950 times. This illustrates that theory is not necessarily less cited than methodology.

As Tables 4 and 5 show, the two most-cited articles for Milstein and Köhler are two of their collaborative works. The first article describing the hybridoma technique[15] is one of the most-cited articles between 1961 and 1982.[22] The second most-cited paper for both Milstein and Köhler, "Derivation of specific antibody-producing tissue culture and tumor lines by cell fusion," was cited over 800 times. This article extends the use of fusion techniques for producing specific antibodies.[24]

Milstein's third most-cited paper, "Antibodies to major histocompatibility antigens produced by hybrid cell lines,"[25] was one of the 1977 articles most cited between 1977 and 1979, with 99 citations.[26] Milstein is included in Table 1 as 1 of the 1,000 contemporary scientists most cited between 1965 and 1978.[27] Our annual lists of most-cited current papers have often included works that were accorded early recognition and give forecasters good reason to watch the related fields for future growth potential.

Köhler's third most-cited paper, cited 427 times, describes a method for making a better tumor-cell fusion partner,[28] and his fourth most-cited paper studies the control of synthesis and secretion of antibodies.[29]

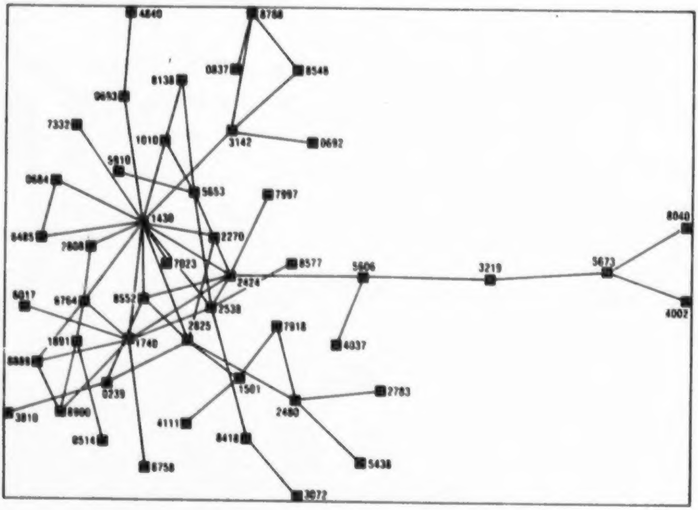
Research-Front Data

Table 6 presents the research fronts in which Jerne's works occur as core documents. Briefly, a research front develops when authors cite a paper to indicate its relevance to their own research. Papers

Table 6: ISI® research fronts in which works by Niels K. Jerne occur as core documents. A = number. B = name. C = number of core papers. D = number of citing papers. Asterisks (*) indicate research fronts that appear on the historiograph in Figure 3.

A	B	C	D
*76-1010	Anti-idiotype antibodies	2	37
*81-0016	Regulation of immune response with emphasis on genetic control	34	423
*82-0317	Growth and regulation of B-cell activation by T cells, helper T cells and antibodies	46	433
83-1491	Major histocompatibility genes of mouse and man: structure, genetics, polymorphism and their role in T-cell immunocompetency	47	931
*83-5673	Expression of T-cell idiotypes and murine antibodies: regulation of T-cell activation	7	229
83-7618	*In vitro* activation of T-cell and B-cell mouse lymphocytes for study of immune responses	3	295
*84-0032	Clinical and experimental studies of immune responses within an idiotype network	10	256

Figure 2: Multidimensional scaled map for #83-0145, "cDNA cloning, gene structure, RNA activity expression and protein structure," showing links between research fronts. A = 1983 research-front number. B = research-front name.


4840
8788
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8138
9693
7332
1010
3142
0692
5810
0684
5653
7997
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8485
7808
2270
7023
8577
5606
3219
5673
8040
4002
2424
6017
6764
8552
2538
2025
7918
1891
1740
4037
6888
0239
1501
2783
2480
3810
8900
4111
8418
0514
5438
6758
3072

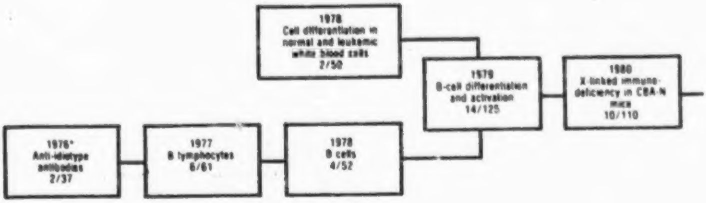
Key

A	B
0239	Mapping and gene structure in *Escherichia coli*, other bacteria and plasmids; *in vitro* transcription and RNA-polymerase isolation
0514	Synthesis, chemical modification and biochemical characterization of histone variants associated with cell differentiation and changes in cell growth

(continued on next page)

A	B
0684	Methods of biosynthesis of DNA and cDNA cloning into *Escherichia coli* in the production of human insulin
0692	Immunochemical identification of proteins after transfer blotting: characterization of DNA binding proteins
0693	Gene sequences and isolation of cDNA clones; characterization and transcription of messenger RNA
0837	Applications of high-resolution 2-dimensional protein electrophoresis to protein phosphorylation and other chemical modifications
1010	Prenatal diagnosis of beta thalassemia by DNA polymorphism; restriction enzyme mapping and structure of globin gene
1430	cDNA cloning from messenger RNA in *Escherichia coli* as probes of virus and eukaryote gene and protein structure
1501	Gene expression, regulation and cloning in *Escherichia coli* K-12 and its mutants
1740	Oncogenes and the genetics of human cancer; viral transforming genes and their DNA structure
1891	Role and arrangement of nucleosomes, histones and other proteins in the organization of the nuclear matrix and the structure of the chromatin DNA
2270	Isolation, expression, cloning and related studies of *Saccharomyces cerevisiae* and other yeast genes and plasmids
2424	Nucleotide sequence of eukaryotic globin genes; characterization by messenger RNA analysis; use of viral gene expression and other *in vitro* models
2480	Transposable genes and TN5 insertion in *Drosophila melanogaster* and *Escherichia coli*; evolution of transposon DNA sequences in eukaryotes
2538	Gene transcription, expression and sequence including protein structure and RNA activity
2783	Transposable gene elements and hybrid dysgenesis in *Drosophila melanogaster*; role in evolution
2808	DNA methylation; sequence structure and effect on gene activity
2825	Molecular cDNA cloning of genes in *Escherichia coli*; nucleotide sequence and protein structure
3072	Transcription of class-III transfer RNA genes by RNA polymerase-III in *Xenopus laevis* and other eukaryotes
3142	Characterization of proteins via immunochemical and biochemical methods; rapid detection and modifications related to activity
3219	Genetics of immunoglobulin formation in mouse; gene rearrangement and antibody diversity and idiotypes
3810	Characterization of *Escherichia coli* RNA polymerase and its interaction with DNA promoters in the regulation of transcription
4002	T-cell mediated regulation of B-cell activation; suppressor, contrasuppressor and antigen specific immune responses in normal and tumor cells
4037	Characterization of gene polymorphism in yeast, *Saccharomyces cerevisiae*; transcriptional and post-transcriptional regulation during development and differentiation in eukaryotes
4111	Genetic analysis of gene expression in *Escherichia coli* and yeast by gene fusions; characterization of mutations of promoter regions

Figure 3: Historiograph showing development of theories of the immune system. Numbers at the bottom of each box refer to the number of cited/citing papers for each research front. An asterisk(*) indicates research fronts in which Jerne is author of a core paper.


1978
Cell differentiation in normal and leukemic white blood cells
2/50
1979
B-cell differentiation and activation
14/125
1980
X-linked immuno-deficiency in CBA-N mice
10/110
1976*
Anti-idiotype antibodies
2/37
1977
B lymphocytes
6/61
1978
B cells
4/52

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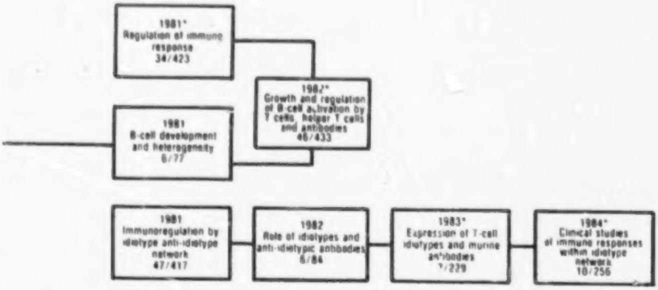
A	B
4840	Characterization of factors affecting messenger RNA synthesis, activity and degradation; gene expression measurement by evaluation of *in vitro* translation products
5438	Transposable DNA sequences and satellite DNA in *Drosophila*: role of repetitive elements in evolution
5606	Processing of messenger RNA of murine immunoglobulin and histocompatibility genes; cDNA probes to study B-cell regulation
5653	Nucleotide sequence of human and mouse genes as tools to study evolution; cDNA for beta globin
5673	Expression of T-cell idiotypes and murine antibodies; regulation of T-cell activation
5910	Analysis of mutagenesis and nucleotide sequence of genes of *Escherichia coli*, yeast and other eukaryotes; mutations by frame-shifting, ultraviolet radiation, repeated sequences, hypervariable sites and slipped mispairing
6017	DNA sequences, properties and mutants of simian virus 40 and polyoma virus
6485	Molecular cloning of human genes; enzymes, fibronectin and histocompatible antigen genes
6758	Avian virus oncogene products; characterization of transforming proteins and induction of lymphoma in chickens
6764	Gene expression and relation to transformation in mammary tumor viruses, adenoviruses and mouse-human cell hybrids
7023	Cloning, isolation, and sequence analysis of cDNA and messenger RNA for genes from humans and other animals
7332	Repetitive DNA sequences in the organization of human genomic families
7918	Transposons of *Escherichia coli* and *Salmonella typhimurium*; mutagenesis and construction of new gene elements
7997	Nucleotide sequence and gene structure of tumor and virus antigens; cap structures of messenger RNA
8040	Cross-reactive idiotypes and genetic basis of antibody formation; heterogeneity of idiotypic mouse monoclonal antibodies
8138	Regulation of human globin genes; endonuclease and structural DNA studies in thalassemia
8418	RNA transcription *in vitro*; initiation and expression and characterization of RNA polymerases
8546	Methods of protein purification and characterization using silver staining and 2-dimensional polyacrylamide-gel electrophoresis
8552	Genetic studies of DNA nucleotide sequences, protein activation, messenger RNA structure and related topics
8577	Transcription of ribosomal RNA genes in *Xenopus laevis*, mice and *Escherichia coli*
8788	2-dimensional gel electrophoresis in the characterization of protein synthesis and expression in cells
8889	DNA-mediated gene transfer and expression of herpes simplex virus thymidine kinase gene in mammalian cells
8900	Expression of genes including the thymidine kinase gene and the stability and inhibition of mammalian cell transformation


1981*
Regulation of immune response
34/423
1982*
Growth and regulation of B-cell activation by T cells, helper T cells and antibodies
46/433
1981
B-cell development and heterogeneity
6/77
1981
Immunoregulation by idiotype anti-idiotype network
47/417
1982
Role of idiotypes and anti-idiotypic antibodies
6/84
1983*
Expression of T-cell idiotypes and murine antibodies
7/229
1984*
Clinical studies of immune responses within idiotype network
10/256

that are frequently cited together, or co-cited, share common features such as topics, results, methods, or discussion. In this way, the citing authors themselves categorize papers into subject-related clusters of research. These co-citation groups help identify research fronts.

Figure 2 shows the multidimensional scaling map for the C2 research front entitled "cDNA cloning, gene structure, RNA activity expression and protein structure" (#83-0145). The map shows how many related C1 fronts are linked by co-citation. One of the core papers for "Expression of T-cell idiotypes and murine antibodies: regulation of T-cell activation" (#83-5673) is the 1974 paper by Jerne.[9]

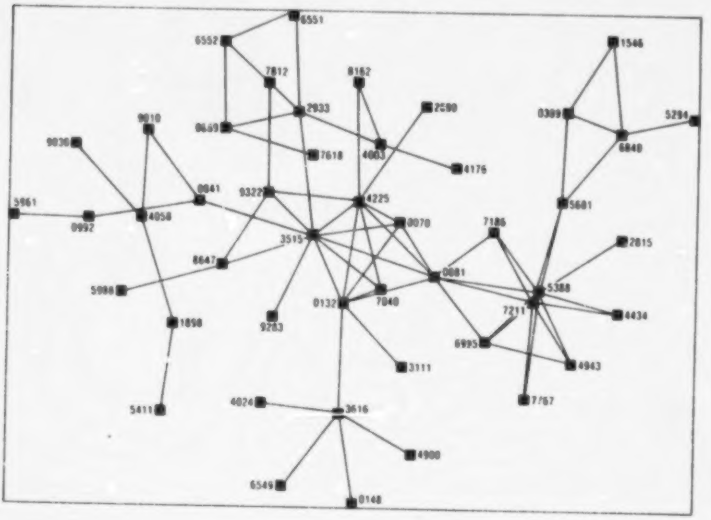
To show how Jerne's field of study has changed and advanced between 1976 and 1984, Figure 3 presents a historiograph (cluster string) of Jerne's contributions to the development and control of the immune system during this period. Each box includes the research-front name, the number of core articles, and the number of citing (published) papers. The fronts that are included are determined by the continuity of the core literature from year to year.[30] If the same core documents are cited at the required thresholds in two adjacent years, then a "string" is established. By continuing this procedure, a historiograph is developed.

Table 7 shows the research fronts that include papers by Milstein and Köhler as core documents. Figure 4 is a higher level map for "Identification of T-cell and B-cell subsets and their role in tumor immunotherapy" (#83-0031). This map shows front #83-0081, "Studies of human natural-killer cells and other T-cell subset activities," which includes a core paper by Milstein.[31]

Table 7: ISI® research fronts in which works by César Milstein and Georges J.F. Köhler occur as core documents. A = number. B = name. C = number of core papers. D = number of citing papers. Asterisks (*) indicate research fronts that appear on the historiograph in Figure 5.

A	B	C	D
74-0003	RNA viruses	490	3573
75-0034	Ribosomes	13	159
*76-0514	Messenger RNA	9	105
*76-0601	Replication of DNA and RNA	21	217
*77-0621	Protein synthesis	12	186
*78-0534	Biosynthetic precursors	11	165
*78-1440	Production of monoclonal antibodies to various antigens	3	99
*79-0363	Hybrid myeloma monoclonal antibody production	6	204
*79-0550	Synthesis of preprohormone precursors	10	174
*79-1716	Sequences of immunoglobulin light-chain genes at recombination sites	4	43
81-0031	Retinoids and cancer	47	519
*81-0054	Protein transfer across membranes	6	270
81-0104	Expression of immunoglobulin genes with emphasis on heavy chain	49	434
*81-0710	Monoclonal antibodies	7	153
81-1664	Production of monoclonal antibody by hybridoma	3	578
*82-0224	Regulation and mechanism of transmembrane translocation of different proteins: role of gene expression and signal peptide sequences in the synthesis, secretion and transport of proteins	41	486
82-1232	Maturation of T-cell subsets and role of T cells in graft vs. host reactions and skin allograft rejection	4	99
83-0081	Studies of human natural-killer cells and other T-cell subset activities	22	546
*83-1880	Monoclonal antibodies in the characterization of HLA antigens, cytotoxic T cells and other cell surface antigens, and their expression	3	149
84-0171	Clinical aspects and characterization of human T-cell subsets	47	1206
84-3435	Monoclonal antibody analysis of rat T cells and their cytotoxic effects in allograft rejection	9	184
84-6624	Biomedical applications of monoclonal-antibody technology	2	68
84-7912	Studies of microtubules using monoclonal antibodies to tubulin	2	40

Figure 4: Multidimensional scaled map for #83-0031, "Identification of T-cell and B-cell subsets and their role in tumor immunotherapy," showing links between research fronts. A = 1983 research-front number. B = research-front name.



Key

A	B
0041	T-cell subset studies of systemic lupus erythematosus and other lymphocyte related diseases
0070	Characterization of epidermal langerhans cells, T cells and others by monoclonal antibodies and other immunological techniques, and studies of histiocytosis-X
0081	Studies of human natural-killer cells and other T-cell subset activities
0132	B-cell and T-cell antigens identified by monoclonal antibody and used in identification of lymphocyte subpopulations and leukemia
0148	Radiolabeled monoclonal antibodies to carcinoembryonic antigen and other antigens to detect and localize tumors via immunoscintigraphy
0309	Human leukocyte interferon gene cloned into *Escherichia coli*; tolerance and antiviral activity of alpha-interferon and gamma-interferon
0889	Regulation of B-cell growth and development by T-cell soluble factors; synergism of immunoglobulin and interleukin
0992	Activity of T-cell subsets in multiple sclerosis and their analysis with monoclonal antibodies
1546	Antiviral and anticellular action and other actions of interferon
1898	Kaposi's sarcoma, cytomegalovirus infection, immunological factors and other aspects of the pathogenesis of acquired immune deficiency syndrome in homosexual men and other populations
2690	Immunofluorescence and other techniques using monoclonal antibodies in the study of T-cell and B-cell lymphocyte, lymphoma leukemia and other human cell antigens
2815	Stimulation of human interferon producing cells by mycoplasma, tumor-cell lines, mediation of natural-killer cytotoxicity
2933	Human T-cell lymphoma virus and adult T-cell leukemia: nucleic acid analysis of virus and expression induced by interleukin-2
3111	Terminal deoxynucleotidyl transferase as immunological markers in human cells with chronic and acute lymphocytic leukemia

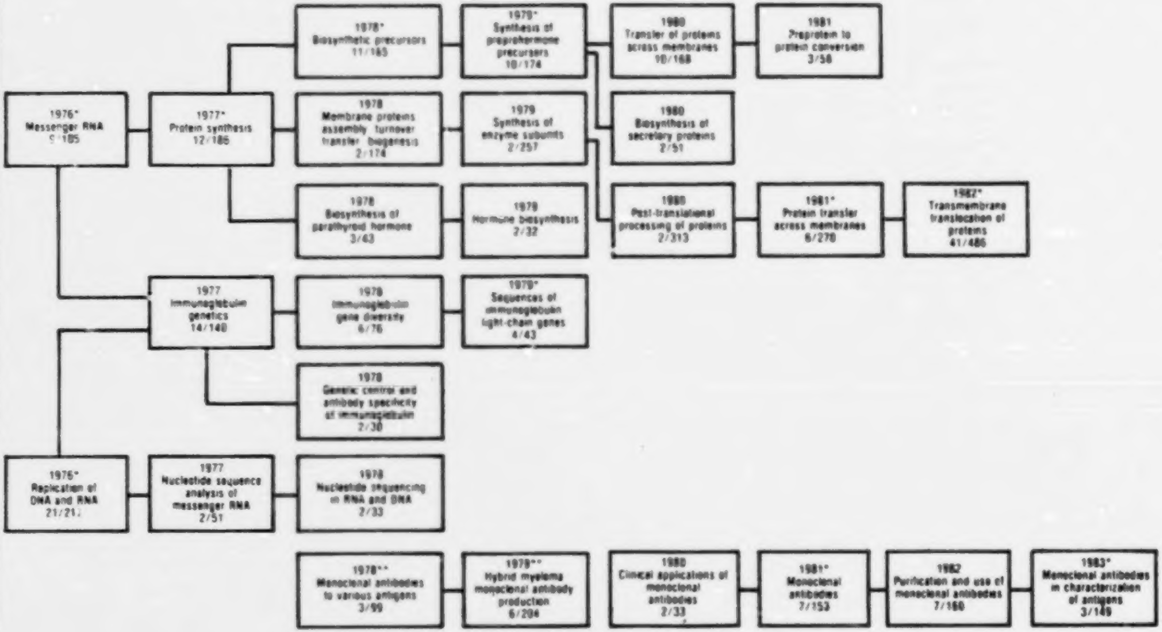
(continued on next page)

A	B
3515	Studies of T-cell subsets in cancer and other diseases by monoclonal antibodies and other techniques
3616	Therapeutic use of monoclonal antibodies in leukemia and other cancers
4003	Cloned cytolytic T cells: functional and immunogenetic analysis of cytolytic lymphocytes T cells and their helper characteristics
4024	Tumor-associated differentiation antigens of human melanoma: analysis of monoclonal antibodies and typing of cell-surface antigens
4058	Circulating T-cell subsets in acquired immune deficiency syndrome, cytomegalovirus mononucleosis and hemophilia: immunoregulatory abnormalities in humans
4176	Mutagens, irradiation antilyt-2 antibodies and other factors in the generation of cytolytic lymphocyte-T precursor clones: applications to tumor immunotherapy
4225	Function of lymphocyte T cells and their antigens including characterization with monoclonal antibodies
4434	Natural-killer cell activity and the presence of sodium-potassium antigens in cytotoxic murine lymphocytes
4900	Generation and characterization of human monoclonal antibody producing hybridomas: applications using lymphocytes from patients with malignant tumors and other disorders
4943	Characteristics and regulation of natural-killer cell activity: role of lymphocytes and other cells in the modulation of cytotoxicity
5294	Production of interferon in lupus erythematosus and its application to acquired immune deficiency syndrome
5388	Natural-killer cytotoxicity of human and mouse lymphocytes affected by interferon and interleukin-2 *in vitro* activity
5411	Acquired immune deficiency syndrome and lymphoadenopathy in homosexuals: morbid anatomy and tumor associations
5681	Effect of recombinant interferon treatment on human natural-killer cell cytotoxicity
5961	Studies of cerebrospinal fluid IGG in multiple sclerosis and encephalomyelitis
5988	Study of human lymphocytes based upon esterase activity and monoclonal antibody reactivity
6549	Monoclonal antibody conjugates in the selective effects of ricin and other toxins
6551	Effect of interleukin-2 on the generation, differentiation and activity of cytotoxic T cells
6552	Characterization of interleukin-2 and its effect on T-cell production in relation to other growth factors
6840	Effect of interferon on human and mouse cells: characterization of antiviral effects and production by human leukocytes
6995	Natural-killer cells and antitumor activity in conjunction with interferon, radiation and chemotherapy: *in vitro* assessment of killer cell suppression
7040	Use of monoclonal antibodies in the characterization of T-cell antigens in the analysis of lymphocyte subsets
7186	Mechanisms of cytotoxicity for human natural-killer cells
7211	Natural-killer cell and cytotoxicity activity and effect on lymphocytes and tumors
7618	*In vitro* activation of T-cell and B-cell mouse lymphocytes for study of immune responses
7767	Studies of natural-killer cell activity, cell-mediated cytotoxicity and resistance to antiasialo GMI antiserum
7812	*In vitro* activity of T cells and B cells including the production of immunoglobulin secreting cells
8162	T-cell surface marker antigens identified with monoclonal antibodies, characterization of expression of cytotoxic clones
8647	Characterization with monoclonal antibodies and other properties of peripheral-blood lymphocyte T cells
9010	Lymphocyte-T suppressor cells and other T-cell subsets after human bone-marrow transplantation
9030	Cytomegalo-virus infection and other opportunistic infections in renal-transplant recipients
9283	Characterization of the receptor for transferrin on T lymphocytes and other cells using monoclonal antibodies and other techniques
9322	Activity, characterization and other studies of human suppressor T-cell subsets

Figure 5 is a historiograph documenting the progress of research and development in monoclonal antibodies between 1976 and 1983. Shifts in research emphasis resulting from new discoveries may cause the flowchart to branch, as in the 1976 messenger RNA front.

In a recent issue of *CC*, I described the *ISI Atlas of Science: Biotechnology and Molecular Genetics, 1981/82**.[32] Among the 127 chapters in this work, 7 are devoted to topics in which the work of Jerne, Milstein, and Köhler figure prominently.

Figure 5: Historiograph tracing research leading to the monoclonal antibody technique. Numbers at the bottom of each box refer to the number of cited/citing papers for each research front. One asterisk (*) indicates research fronts in which Milstein is an author of a core paper; two asterisks (**) indicate research fronts in which both Milstein and Köhler are authors of core papers.


1976*
Messenger RNA
9/185
1977*
Protein synthesis
12/186
1978*
Biosynthetic precursors
11/165
1979*
Synthesis of
preprohormone
precursors
10/174
1980
Transfer of proteins
across membranes
10/168
1981
Preprotein to
protein conversion
3/58
1978
Membrane proteins
assembly turnover
transfer biogenesis
2/174
1979
Synthesis of
enzyme subunits
2/257
1980
Biosynthesis of
secretory proteins
2/51
1978
Biosynthesis of
parathyroid hormone
3/43
1979
Hormone biosynthesis
2/32
1980
Post-translational
processing of proteins
2/313
1981*
Protein transfer
across membranes
6/270
1982*
Transmembrane
translocation of
proteins
41/486
1977
Immunoglobulin
genetics
14/140
1978
Immunoglobulin
gene diversity
6/76
1979*
Sequences of
immunoglobulin
light-chain genes
4/43
1978
Genetic control and
antibody specificity
of immunoglobulin
2/30
1976*
Replication of
DNA and RNA
21/212
1977
Nucleotide sequence
analysis of
messenger RNA
2/51
1978
Nucleotide sequencing
in RNA and DNA
2/33
1978**
Monoclonal antibodies
to various antigens
3/99
1979**
Hybrid myeloma
monoclonal antibody
production
6/204
1980
Clinical applications of
monoclonal
antibodies
2/33
1981*
Monoclonal
antibodies
7/153
1982
Purification and use of
monoclonal antibodies
7/160
1983*
Monoclonal antibodies
in characterization
of antigens
3/149

The experienced reader may not need to be told how rapidly the field of immunology has grown in the past dozen years. From the basic research on immunity to infectious diseases, we now realize that the immune system encompasses many other biological topics including genetics, transplantation, cancer, and cell differentiation. But our maps do point to the primordial years when modern immunology advanced as a result of a few unifying principles and techniques. These form the background for an entirely new set of disciplines. I've simply tried to give you another perspective on the contributions of Jerne, Milstein, and Köhler to this remarkable chapter in modern research.

* * * * * * *

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